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PERSONAL TEMPO [1]

BY HORACIO J. A. RIMOLDI

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AMONG the variables we consider when judging or characterizing an individual, his natural speed occupies a foremost position in our picture. Briefly stated our problem is to determine the factors related to the concept of *personal tempo*.

The present study was carried on by: (*a*) analyzing speed of motor activities (large and small movements, proximal and distal, symmetric and parallel, left and right side movements, upper and lower limbs, and so on), (*b*) reaction time measurements (simple discrimination and discrimination and choice), (*c*) complex processes such as judgments, recognition of designs and so on, (*d*) expressive movements like drawing, etc., (*e*) intellectual processes such as those involved in the performance of part of the tests of the *Primary Mental Abilities* (PMA). In addition other tests like preferred metronome rate, pulse rate, and speed of walking were also introduced.

An exact evaluation of the existing bibliography as a whole is quite difficult.[2] Each author had a particular problem in mind and proceeded accordingly to explore the field. What seems to be a serious handicap is the looseness with which terms have been employed and the heavy theoretical bias pervading some studies. Words like *tempo, level, altitude,* imply quite unrelated concepts in the writings of different psychologists.

It is obvious that if the interest is to prove or disprove the existence of a general factor of personal tempo, we cannot be satisfied with the analysis of a few similar or somewhat related activities, no matter how keen our intuition may be or how extensive the number of subjects employed. The planning of the experiment should be done in such a way as to increase the odds against any spurious system of correlations; that is, the functions explored should be of different kinds, involving varied sorts of performances and so on.

When talking of speed in relation to certain activities some authors refer to spontaneous speed, others to maximal speed, and still others to experiments on leisure. Some authors do not state whether the subjects (*Ss*) were asked to work at their natural or at their maximal speed. It is our belief that, if we are searching for a study of the individual as he behaves in ordinary life, spontaneous speed should be used. There are certain measurements like reaction time that should be performed at maximal speed; otherwise the purpose of the test would be altered.

In the instructions no mention should be made of the word *speed*. Allport and Vernon (2) insist on this point. The subject should be asked to perform the tests in his most "natural, congenial way," or for the purpose any equivalent expression may be used.

[1] This study was initiated in the fall of 1947 at Harvard University and continued from February, 1948, onwards at the University of Chicago's Psychometric Laboratory, where it was completed in the summer of 1949.

From September, 1947, to September, 1948, the study was carried out under the auspices of the John Simon Guggenheim Foundation, of which the author was a Fellow and to whom he is greatly indebted for their wholehearted support. Since October, 1948, the completion of the research was made possible by the help and consideration afforded by the United States Public Health Service, of which the author is at present a Special Research Fellow.

The author is very grateful to Dr. G. W. Allport and to the Department of Social Relations at Harvard University for the intellectual and financial help received. It has been a privilege to count on Dr. Allport's criticism and scientific help. The Harvard Psychological Laboratory provided most of the equipment necessary for this study, for which the author expresses his appreciation.

The intellectual advice and strictly scientific criticism that Dr. L. L. Thurstone has given during the development of this research has been of utmost value.

The author wishes also to express his thanks to those Harvard students who willingly volunteered as subjects, and to Dr. Helen Heath who helped with part of the computational work and, together with Mr. Vernon Tracht, aided in preparing the final form of the manuscript.

[2] The historical aspect of the problem is treated by the author in a separate article.

We favor the use of tests that are simple and easily scored. By *simple* we mean tests that, as a result of previous investigations, are known to be good indicators of well-defined functions. It is obvious that no amount of statistical elaboration will supply the information desired unless the variables are selected so as to explore specifically or preferentially the function under consideration. If some well-known tests are included as landmarks, other variables, as many as the psychologist can invent, may be included in the battery.

The simpler variables are probably the more difficult to obtain. This study reveals that a few of our tests have failed to meet this criterion but that effort in this direction is worth trying.

Although devising *new* tests of intelligence seems to be quite fashionable, originality in this field being overwhelming, it was thought safer to use the PMA's because a great deal of experimental work has been done on them. Some tests like tapping, metronome, ergograph, speed of walking were included to test previous hypotheses in the field.

A considerable number of our tests involve motor activity. The nature of these tests shows that they have been selected according to the precautions mentioned.

Unfortunately, it was impossible, for practical reasons, to include all the tests required for a thorough survey of the field, but for our purpose the ones employed represent quite an acceptable sample. Tests like writing with the hands and feet were introduced in the hope of obtaining information on the problem of speed in expressive movements. Tests of a physiological nature were duly considered, but for reasons of practicability and in view of the mainly exploratory nature of this study, these, excluding pulse rate, were eliminated. In further studies they should be carefully considered.

It is recommended that tests having a high probability of success be employed. Slater (33) has computed only those items that were perfectly solved and gives acceptable reasons for following such a method. This is undoubtedly a limitation, but in the present stage of our inquiry it is better to do so. It is possible to find numberless variations in complexity with a 100 per cent probability of success. When conditions permit, graphic record of the performances should be kept. We did so for some of the tests until the evidence gathered proved it to be unnecessary.

Critics may find these conditions either too naïve or too complex, or believe that the way of attacking this particular problem is wrong or biased. We know that to try to be too exacting might be a dangerous thing, but we are also aware of the danger of impressionistic prose. The true scientific attitude seems to rest in a balanced coordination of good hypotheses combined with careful and precise observations and experiments.

METHOD AND DESCRIPTION OF THE TESTS

All the tests were so selected that they could be successfully accomplished by all our *Ss*, who were 91 male students between 19 and 25 years of age at Harvard University. The names and a brief description of the tests are presented in Table 1.[3]

The tests were given individually. With the exception of two *Ss*, all completed the entire battery in a single session of approximately three hours' duration. Seventeen of the *Ss* repeated all the tests within a period varying from two to four weeks. For 25 *Ss* graphical records were kept of the performances in tests 1 to 16 inclusive. In addition, time was recorded for each interval of five seconds.

For the tests of tapping, a telegraph key was employed. In those cases in which no graphic record was kept, the number of movements was counted either directly or by means of an electric counter. Each test performance lasted for 30 seconds, except where otherwise stated in Table 1. All the tests, except 44 to 51 inclusive, were performed in the most "natural, congenial way."

The scores for tests numbered 1 to 17 and 20 and 21 were the numbers of complete movements. By a *complete movement* we mean a return to the previous initial position.

QUANTITATIVE TREATMENT OF THE GROUP RESULTS

Measures of Reliability

Strictly speaking, not all the values listed should be understood as reliability coefficients. We first studied the correlations obtained by means of the test-retest procedure with 17 *Ss*. Other things being equal, these

[3] For more detailed descriptions of these tests and for the table of intercorrelations, order Document 3304 from American Documentation Institute, 1719 N St., N.W., Washington 6, D.C., remitting $1.00 for microfilm (images 1 in. high on standard 35 mm. motion picture film) or $1.50 for photocopies (6 x 8 in.) readable without optical aid.

TABLE 1

A LIST OF THE TESTS USED *

TEST No.	DESCRIPTION
1.	Swinging right arm: standing
2.	Swinging left arm: standing
3.	Symmetrical movement arms: standing
4.	Parallel movement arms: standing
5.	Swinging right leg: standing
6.	Swinging left leg: standing
7.	Symmetrical movement legs: seated
8.	Parallel movement legs: seated
9.	Tapping right heel: standing
10.	Tapping left heel: standing
11.	Tapping right toe: standing
12.	Tapping left toe: standing
13.	Tapping right arm: seated
14.	Tapping left arm: seated
15.	Tapping right finger: seated
16.	Tapping left finger: seated
17.	Ergograph: seated
18.	Counting 1 min.: two 30-sec. periods
19.	*Sorting cards*: repeated
20.	Finger to nose: standing
21.	Bending body: seated
22.	Marbles right: two 15-sec. periods†
23.	Marbles left: two 15-sec. periods†
24.	Right foot lines: two 15-sec. periods‡
25.	Right foot circles: two 15-sec. periods‡
26.	Right foot squares: two 15-sec. periods‡
27.	Left foot lines: two 15-sec. periods‡
28.	Left foot circles: two 15-sec. periods‡
29.	Left foot squares: two 15-sec. periods‡
30.	Copying, 1 min.§
31.	Writing *e*'s, 1 min.§
32.	Cancellation, 1 min.§
33.	Number of things‖
34.	Reading literature, 1 min.: two 30-sec. periods
35.	Reading science, 1 min.: two 30-sec. periods
36.	Reading news, 1 min.: two 30-sec. periods
37.	Pulse rate, 1 min.
38.	*Speed of walking*¶
39.	*Verbal meaning***
40.	*Space***
41.	*Reasoning***
42.	*Number***
43.	*Word fluency*‡‡
44.	*Reaction time to sound*§§
45.	*Reaction time to light*§§
46.	*Discrimination time*§§
47.	*Discrimination and choice time*§§
48.	*Recognition*§§
49.	*Equality and differences*§§
50.	*Free association*§§
51.	*Like or dislike*§§
52.	Writing lines, right hand, 1 min.: two 30-sec. periods‡
53.	Writing circles, right hand, 1 min.: two 30-sec. periods‡
54.	Writing squares, right hand, 1 min.: two 30-sec. periods‡
55.	Writing lines, left hand, 1 min.: two 30-sec. periods‡
56.	Writing circles, left hand, 1 min.: two 30-sec. periods
57.	Writing squares, left hand, 1 min.: two 30-sec. periods
58.	Metronome first‖ ‖
59.	Metronome second‖ ‖

* All tests in italics were scored in terms of time of performance; the others were scored in number of performances per unit of time.

† Transferring marbles from tray at right to tray at left with right hand for 15 seconds. The reverse process was repeated for 15 seconds. Test 23 was identically performed with the left hand.

‡ Tests 24 to 29 involved making the indicated designs on a tray of sand while *S* was standing. By means of a leather strap a nail was attached to the shoe. Tests 52 to 57 involved sketching the designs on a sheet of paper of standard size.

§ In tests 30 to 32 a record was kept of the accomplishment during the first and second half of the one-minute period.

‖ Subject enumerated items observed in a picture of a traffic accident.

¶ Test 38 was scored as time per step.

** Tests 39 to 49 are those of *Primary Mental Abilities* (PMA's). Time was recorded for each individual item, and the final score was the total of these.

‡‡ Score was time required to write 20 words beginning with *P*.

§§ Tests 44 to 51 were performed at maximum speed. The score was the average time of 20 measurements, with the exception of test 46, which was based on 10 trials. Test 48 implied recognition of a simple geometrical figure (square, circle, oval, cross, and triangle). Test 49 implied judging the equality or difference of the previously-mentioned figures when presented two at a time. Test 51 involved affective judgment on Brunswik's faces. A voice key was employed to record time of response in tests 48 to 51.

‖ ‖ Tests 58 and 59 were scored as preferred metronome rate.

correlations indicate the consistency of the individual's tempo at periods separated by intervals between two and four weeks. The values obtained are given in Table 2.

Although these coefficients are based on 17 cases, their high value is extremely suggestive and for all practical purposes can be considered as very satisfactory. It is customary in studies of this kind to report values which are well below the ones here obtained. These coefficients indicate that, at least in our experimental conditions and for our sample of *S*s, spontaneous speed in various kinds of performance is a highly stable characteristic.

It might be expected that those individuals with better integration or adjustment would be less subject to variation within the course of time than would be individuals with poor integration or adjustment. This hypothesis should be tested.

TABLE 2

TEST-RETEST RELIABILITY *

TEST	RELIABILITY	TEST	RELIABILITY	TEST	RELIABILITY	TEST	RELIABILITY
1	.78	16	.88	31	.89	46	.80
2	.90	17	.75	32	.65	47	.58
3	.73	18	.85	33	.73	48	.47
4	.84	19	.96	34	.83	49	.37
5	.71	20	.78	35	.70	50	.58
6	.73	21	.75	36	.76	51	.50
7	.77	22	.90	37	.77	52	.85
8	.83	23	.87	38†		53	.92
9	.88	24	.89	39	.70	54	.93
10	.91	25	.79	40	.72	55	.91
11	.89	26	.79	41	.62	56	.77
12	.90	27	.71	42	.83	57	.87
13	.92	28	.92	43	.44	58	.73
14	.91	29	.83	44	.69	59	.88
15	.87	30	.52	45	.42		

* These rank-order correlation coefficients were obtained by retesting 17 of the 51 within a period varying between 15 and 30 days.
† Disturbing factors beyond our control prevented us from obtaining a reliable score on the retest of variable 38, speed of walking.

Our results as well as those of previous authors tend to support the hypothesis of individual constancy in tempo measurements over periods of considerable time.

The scores of those tests that were repeated twice during a single experimental session were also correlated. The values obtained, as shown in Table 3, are consistently high; all but one are above +.80, and the majority are above +.90.

The Intercorrelations and the Analysis

In treating these data, the original raw scores were transformed into single digits scores. Tests 19, and 38 to 51 inclusive were scored in terms of time of performance instead of in number of performances per unit time. Consequently the signs of the corresponding correlation coefficients were reversed. Of 1,711 correlations (half of the table) over one-third are significant at the 1 per cent level of confidence; less than one-eighth are negative, and of these only two reach significance at the 1 per cent level. Inspection of the table of intercorrelations [4] reveals well-defined clusters of high values.

[4] Available as part of Document 3304 from the American Documentation Institute. See footnote 3 above.

TABLE 3

RELIABILITIES FOR TESTS REPEATED WITHIN AN EXPERIMENTAL SESSION *

TEST	RELIABILITY	TEST	RELIABILITY	TEST	RELIABILITY
18	.96	28	.93	36	.91
19	.92	29	.84	52	.94
22	.98	30	.82	53	.95
23	.99	31	.83	54	.91
24	.96	32	.77	55	.95
25	.90	34	.88	56	.96
26	.87	35	.86	57	.96
27	.96				

* These coefficients were obtained by correlating the scores of tests repeated during a single experimental session on each of the 91 subjects.

TABLE 4

THE CENTROID FACTORIAL MATRIX *

TEST NO.	I	II	III	IV	V	VI	VII	VIII	IX	h^2
1	61	41	−11	26	06	30	22	−08	11	78
2	55	45	−09	29	04	29	30	−07	15	80
3	60	48	−25	26	−16	−03	13	10	07	78
4	47	41	−18	33	−27	−09	18	07	12	66
5	67	34	−04	30	−17	18	24	10	11	80
6	64	37	04	20	−14	18	28	09	09	73
7	70	45	06	25	−16	15	06	04	09	82
8	58	41	02	33	−25	04	17	07	08	72
9	66	50	12	−14	22	10	−22	13	−06	85
10	67	49	18	−16	25	05	−22	09	−12	88
11	70	44	22	−28	24	05	−19	07	05	91
12	68	42	22	−33	27	07	−15	11	06	91
13	65	44	29	−26	31	−07	−12	10	23	95
14	64	48	22	−20	33	−09	−14	07	17	90
15	69	45	24	−26	17	−13	−18	11	15	92
16	68	40	21	−29	22	−10	−19	08	11	86
17	60	16	−06	17	10	−12	14	09	−14	49
18	48	30	−11	−11	−06	−11	−09	−10	15	40
19	46	−14	−27	−16	11	27	09	−11	−06	44
20	69	35	−24	26	−22	−05	−09	−19	−20	86
21	59	40	−22	22	−16	−12	15	−09	05	68
22	54	09	−07	−28	12	−31	22	−37	−18	71
23	53	09	−07	−32	14	−28	21	−39	−14	71
24	58	−24	36	31	09	22	−09	−22	−12	75
25	59	−22	50	38	18	17	13	−05	−06	88
26	58	−24	49	40	17	21	07	05	−05	88
27	57	−29	32	29	13	16	−10	−24	−13	72
28	54	−24	36	38	26	15	18	−03	−12	76
29	60	−25	45	35	17	14	10	07	−10	82
30	16	−35	−09	−34	−04	−15	−10	29	09	40
31	41	−17	12	−26	−14	−08	−10	10	−17	35
32	36	−24	−15	−10	02	09	05	21	−25	34
33	40	−16	−05	−14	06	10	−04	−21	14	29
34	54	−26	−28	−41	04	15	32	17	14	78
35	49	−22	−28	−41	19	12	27	13	16	70
36	44	−21	−28	−45	21	11	32	20	11	73
37	18	03	13	09	03	−18	−06	−23	−16	17
38	−08	−04	−05	−18	21	24	08	06	19	19
39	20	−31	−38	−24	−02	06	10	−15	−13	39
40	26	−14	−22	17	−16	12	−28	−14	14	32
41	18	−45	−18	−08	−08	19	−04	−32	18	45
42	29	−39	−43	−09	22	15	−12	−19	−09	56
43	21	−27	−24	02	−12	−02	−20	15	−04	25
44	11	−16	−17	23	21	−22	−16	15	−14	28
45	26	−08	−20	27	18	−57	−08	23	−17	63
46	19	−34	−25	16	21	−47	02	26	−08	58
47	23	−39	−29	17	23	−33	−08	26	15	58
48	33	−29	−37	06	09	09	−21	−16	20	46
49	19	−27	−49	21	−12	17	−32	−08	07	55
50	29	−16	−32	14	10	−06	−27	−26	14	41
51	20	−31	−13	12	−08	03	−31	12	11	30
52	40	−31	37	−17	−45	−16	−07	−07	19	70
53	49	−26	46	−21	−36	−14	19	13	−09	77
54	41	−30	47	−20	−34	−21	15	09	−13	73
55	31	−36	33	−10	−34	−13	06	−25	28	62
56	43	−40	38	−20	−30	−06	25	−16	−13	73
57	36	−40	38	−13	−25	−08	31	−11	−03	63
58	25	16	−31	−22	−29	27	−31	09	−32	60
59	35	24	−28	−28	−32	31	−23	18	−34	74

* The decimal point has been omitted for all entries.

It is interesting to observe the predominance of positive values among the intercorrelations, a fact not always reported in personality studies. In interpreting this fact, one must bear in mind the particular kind of problem we have studied and the characteristics of our tests and of their administration.

The table of correlations was factored by the centroid method. Nine factors were obtained. The centroid loadings and the communality of the tests are presented in Table 4.

Several rotational procedures were employed, some of them being the ones described by Thurstone (39), and others being techniques still under experimentation.

The transformation matrix is given in Table 5. The final oblique factor matrix is given in Table 6, and the correlations among the primary factors appear in Table 7. The second order factors were analyzed and interpreted, the implication being that the study of the correlations between factors probably represents more fundamental factors. Few studies have been published on this type of analysis. The hypothetical concepts have been discussed by Thurstone (39). The author reported in several studies (25, 26) the psychological findings and implications of this method of investigation. Another interesting illustration of the application of the second order can be found in an unpublished thesis by H. Heath.

As shown by the loadings the factors are clearly indicated and the structure is quite compelling. It is obvious that with a larger sample than the one here studied, a still closer fit could be expected. But the high reliabilities of our tests and the clusters of very high values for each factor point to a clear and definite structure. Two of the factors seem to be doublets. Doublets, however, have an interesting function in pointing to new dimensions to explore in further studies.

TABLE 5

THE FINAL TRANSFORMATION MATRIX *

	A	B	C	D	E	F	G	H	I
I	33	26	26	14	28	13	17	19	17
II	32	23	−28	02	−29	02	−18	−28	−25
III	−35	22	−43	−18	44	−15	−26	33	−11
IV	38	−50	−41	−10	45	−14	33	−23	08
V	−48	40	27	09	22	−31	10	−73	−18
VI	−06	−13	11	−36	43	30	−69	−31	−04
VII	40	−46	56	10	08	−37	−12	−06	−52
VIII	09	14	−31	−75	02	16	51	09	−36
IX	33	41	03	−47	−44	−77	−04	27	67

* The decimal point has been omitted for all entries.

THE FACTORS AND THEIR INTERPRETATION [5]

Factor A

		Factor A
Test 1	Swinging right arm	.54
Test 2	Swinging left arm	.59
Test 3	Symmetrical movement arms	.70
Test 4	Parallel movement arms	.73
Test 5	Swinging right leg	.67
Test 6	Swinging left leg	.60
Test 7	Symmetrical movement legs	.57
Test 8	Parallel movement legs	.66
Test 17	Ergograph	.31
Test 20	Finger to nose	.51
Test 21	Bending body	.63
Test 18	Counting	.24

All the tests here included involve large movements of the limbs or the trunk, tests 17 and 18 excluded. Tests involving small movements, performed mainly by the use of the distal muscles, do not show any saturation in this factor. The same applies to the rest of the battery.

Apparently, this factor is related to the

[5] For the various factors, only tests showing loadings above .20 will be listed. The interpretations are based mainly on loadings above .30.

TABLE 6

THE ROTATED FACTORIAL MATRIX *

TEST NO.	A	B	C	D	E	F	G	H	I
1	54	02	19	01	22	−04	−12	−22	00
2	59	−03	17	−02	20	−11	−14	−22	−04
3	70	00	02	01	−03	04	20	−01	00
4	73	−11	−05	−01	−06	−04	21	10	04
5	67	−05	05	−10	22	01	04	05	00
6	60	00	08	−09	21	−01	−03	05	−07
7	57	11	−08	−06	20	06	00	07	06
8	66	−05	−10	−06	15	01	09	12	00
9	10	55	−06	−01	13	21	00	−13	−05
10	01	56	−06	06	17	22	00	−12	−08
11	01	67	02	00	08	09	−06	−02	02
12	00	69	06	−03	07	07	−07	−03	−01
13	04	75	01	−07	−01	−15	00	02	07
14	06	71	00	00	−02	−11	04	−05	05
15	10	70	−04	−02	−04	−02	07	12	09
16	04	70	01	02	−02	01	04	07	08
17	31	04	13	14	20	07	27	−06	−13
18	24	30	09	13	−17	−01	05	11	21
19	08	04	48	16	15	17	−12	−12	05
20	51	−07	02	34	11	30	14	00	10
21	63	−02	10	20	−03	00	16	03	06
22	08	15	49	62	−05	−01	02	03	−06
23	06	19	52	61	−07	−03	−02	02	−03
24	00	00	00	12	65	10	−09	04	16
25	02	−02	00	−02	77	−07	00	04	00
26	02	00	−07	−12	76	−03	03	04	01
27	−08	00	04	17	66	09	−04	03	18
28	02	−07	09	03	73	−06	05	−08	−08
29	02	00	−02	−06	74	00	09	05	−04
30	−15	24	15	−15	−13	06	26	30	12
31	−08	17	07	07	12	27	09	32	01
32	−02	−04	24	02	25	31	16	01	−15
33	02	18	30	13	06	−04	−11	08	28
34	20	15	66	−05	−05	00	02	13	01
35	11	24	66	−01	−06	−07	03	01	02
36	08	22	67	−04	−07	−05	04	−01	−08
37	−05	00	−03	30	13	04	05	02	03
38	−10	15	20	−18	−04	−14	−18	−15	00
39	00	−12	50	27	−05	17	−02	−01	05
40	17	00	−01	01	02	10	04	10	43
41	−01	−06	34	14	04	−04	−17	14	41
42	−12	00	47	26	10	16	04	−20	21
43	05	−02	03	−05	01	21	25	14	17
44	−06	−01	−06	06	14	07	44	−14	00
45	11	02	−07	17	−01	00	74	−03	−06
46	03	−02	12	08	00	−08	68	00	−02
47	07	09	13	−08	−04	−17	62	02	20
48	09	09	27	08	−01	−02	11	−04	45
49	16	−13	08	01	00	20	14	−04	43
50	08	09	15	21	−05	−04	15	−06	45
51	02	06	−08	−16	07	11	23	16	34
52	13	14	−05	−02	01	−03	00	74	36
53	11	04	04	−02	24	10	05	64	−06
54	00	02	01	04	22	11	08	64	−05
55	10	05	09	07	04	−21	−09	62	40
56	00	−10	25	20	29	07	−11	53	01
57	04	−11	23	12	27	−06	−07	50	01
58	06	02	−02	04	−02	65	−10	01	03
59	13	05	01	−01	−03	68	−12	04	−12

* The decimal point has been omitted for all entries.

spontaneous speed with which large movements are performed. This speed seems to be independent of the muscular groups concerned (shoulder, hips, trunk) and of the kind of movement (abduction and adduction, swinging, flexion of the body, and so on).

Most of the performances involved in these tests play an important part in relation to gait and gesture. Wilssmann, as quoted by Allport (1), suggests speed as one of the measurable characteristics of gait. Kreezer and Glanville (18), Schwartz and associates (30, 31, 32), and others have described methods for obtaining reliable records of an individual's gait. Outstanding is the contribution of Wolff (42, 43) on the recognition of oneself and others by using photographs of hands, faces, and the whole person clad in a loose garment with the head covered.

The muscles involved in the performances represented in factor A are probably those that contribute more markedly to the gait of the individual. We do not speak of gait when referring to small movements of the fingers or hands, and it is a fact that it is possible to recognize a friend at a distance by the particular way in which he walks.

Both clinical and experimental lesions of the cerebellum produce, among other symptoms, disturbances of gait. In the so-called "trunk ataxia" the incoordination affects the extremities and the trunk (12). The abnormal gait shown by certain patients with cerebellar lesions is well known clinically. Such evidence suggests the possibility that Factor A may be related to some underlying neurological structures.

On the other hand, it is easy to be misled in this field of expressive movements. Many factors may be influential. For instance, Enke, as mentioned by Allport and Vernon (2), indicates that pyknics tend to be slower in their movements than leptosomes or persons of athletic build. Schroedersecker (29) reports similar findings. Other factors are of a more temporary kind, such as emotional conditions, mood, fatigue, etc., and still others are of a cultural nature (1). It is possible that a careful study of the physiological laws governing the movements involved in the tests contributing to defining factor A could add valuable information. We are referring mainly to such studies as those carried on by Marey, and the more recent ones by Elftmann (10).

It is curious to find that test 38, speed of walking, is absent from this factor. It was difficult, in spite of the precautions, to standardize the conditions under which this test was given. In the centroid matrix speed of walking appears as one of the variables with lowest communality. Harrison and Dovens (16) and Harrison (15) included walking in their studies and reported negative correlations with other tests like tapping and so on.

In general terms, factor A seems to indicate the speed of large movements of trunk and limbs, seems to be independent of side of the body, muscular group, or function, and is probably related to what is referred to as gait and gesture of an individual. The analysis of expressive behavior should be a matter of further research.

TABLE 7

CORRELATIONS BETWEEN THE PRIMARY VECTORS *

	A	B	C	D	E	F	G	H	I
A	1.00								
B	51	1.00							
C	−13	−03	1.00						
D	26	28	−43	1.00					
E	29	37	05	18	1.00				
F	14	11	26	−19	−12	1.00			
G	−18	−08	24	−07	06	09	1.00		
H	−10	06	10	10	19	−12	−04	1.00	
I	−12	−27	13	−14	03	27	12	−33	1.00

* The decimal point has been omitted for all entries below 1.00.

Factor B

	Factor B	
Test 13	Tapping right arm	.75
Test 14	Tapping left arm	.71
Test 15	Tapping right fingers	.70
Test 16	Tapping left fingers	.70
Test 12	Tapping left toes	.69
Test 11	Tapping right toes	.67
Test 10	Tapping left heel	.56
Test 9	Tapping right heel	.55
Test 18	Counting	.30
Test 30	Copying	.24
Test 35	Reading science	.24
Test 36	Reading news	.22

Factor B is defined by the tests involving small movements. These movements are performed by means of the distal muscles of the limbs, and seem to be independent of side of the body and of the muscular groups involved. All the more heavily loaded performances included in factor B require a finer coordination and precision of movements than the variables included in factor A.

This may have a genetical counterpart. For instance, a small child can move his limbs and trunk but performs badly the more complex activities required in tapping with fingers, toes, or heels. All the tests requiring gross movements have zero loadings in this factor. The loading for test 30, copying, can well be explained in terms of the movements required in writing, but tests 35 and 36 are more difficult to interpret, although speed of oculomotor movements involved in reading may be related to speed of small movements.

Factors A and B point to the existence of different natural speeds for two groups of movements, one involving the large muscles of the trunk and proximal parts of the limbs, the other involving the distal parts of both extremities. This seems to have some possible explanation in terms of genetic, neurological, and pathological findings.

The correlation between factors A and B is .51, which qualifies the possibility of interpreting these factors as completely independent and suggests some sort of generality of speed for many varied muscular activities. Moreover, the inspection of our table of correlations shows that the coefficients between tests 9 to 16, and tests 1 to 8, and 17, 20, and 21, are positive and high, with an average of .44.

Factor E

	Factor E	
Test 25	Right foot circles	.77
Test 26	Right foot squares	.76
Test 29	Left foot squares	.74
Test 28	Left foot circles	.73
Test 27	Left foot lines	.66
Test 24	Right foot lines	.66
Test 56	Left hand circles	.29
Test 57	Left hand squares	.27
Test 32	Cancellation	.25
Test 53	Right hand circles	.24
Test 54	Right hand squares	.22
Test 1	Swinging right arms	.22
Test 5	Swinging right leg	.22
Test 6	Swinging left leg	.21
Test 2	Swinging left arm	.20
Test 7	Symmetrical movement legs	.20
Test 17	Ergograph	.20

All the tests related to making simple drawings with the feet appear as heavily loaded in factor E. This is independent of the foot used or of the kind of pattern made—lines, circles, or squares. No problem of great accuracy is involved in these performances. Therefore, it seems that this factor is of a rather general kind and that, for the purposes of further testing, any of the variables listed as heavily loaded in factor E could be taken as a reliable measure of drawing with the feet.

It was interesting to find the parallelism of the scores (number of items drawn) for the first and second performance with each foot, independent of the direction of the lines, their size, patterning, and so on. For instance, one time the subject would draw horizontal lines of a certain length; next time he would draw vertical lines, or lines in the shape of a diamond or a square with its diagonals, and so on. The same observation applies to making circles and making squares.

These facts, which point to some important characteristics of performance, will be dealt with in the discussion of factor H.

Variables 25 to 29 form a very definite cluster, the lowest correlation coefficient within the group being .63. The average intercorrelation between tests 24 to 29, with tests 1, 2, 3, 5, 6, 7, 27, 32, 53, 54, 56, and 57 is .36. It was expected that some of these tests would cluster in one of our factors, and such was the case.

Since drawing with the feet involves large muscles, the subject having to move the whole limb, it is easy to understand why variables like 5, 6, and 7, and 1, 2, and 17 are also loaded in factor E. The correlation between factors A and E is .29.

While the tests of tapping do not show any loading in factor E, the correlations between drawing with the feet and different tapping tests is in the neighborhood of .30. This may explain the correlation of .37 between factors B and E. Factors E and H have a correlation of .19, which we shall study later.

Factor H

		Factor H
Test 52	Right hand lines	.74
Test 53	Right hand circles	.64
Test 54	Right hand squares	.64
Test 55	Left hand lines	.62
Test 56	Left hand circles	.53
Test 57	Left hand squares	.50
Test 31	Writing *e*'s	.32
Test 30	Copying	.30
Test 1	Swinging right arm	−.22
Test 2	Swinging left arm	−.22
Test 42	Number	−.20

This factor shows a definite cluster of high values for variables 52 to 57 inclusive. It repeats for the hands the same pattern that factor E indicates for the feet. In both cases it does not matter whether lines, squares, or circles are drawn, or whether the performance is accomplished by means of the right or left hand or foot. All of these measures are highly reliable (test-retest reliability), and for practical purposes any one of these is a good representative of the cluster.

Factors E and H are correlated to a certain degree (.19), and the average intercorrelation of tests 52 to 57 inclusive, with tests 24 to 29 inclusive, has a value of .36.

Allport and Vernon (2) report findings somewhat similar to these. They give a correlation of .42 between foot drawing and hand drawing. In view of these results the authors speak of a drawing speed composite, including drawing figures on paper, foot drawing, and blackboard drawing. Our findings are in agreement to a certain extent, although our results point to two linearly independent factors for the performances. The statistical dependence found both in Allport and Vernon's study and in ours is quite suggestive.

Another point to remember in connection with factors E and H is the following: each subject had two scores for lines drawn with the right hand or foot, two for lines drawn with the left hand or foot, and the same for the other performances. The consistency of the raw scores was remarkable in the great majority of the cases, and for the purposes of computing the correlations the two measures for each particular design and for each hand or foot were combined.

Nevertheless, the inspection of these drawings shows an unexpected variety in patterns for the same individual in the first and second trials. Within limits, changes in size, placement, and pattern would not alter the number of figures drawn, whether in slow, fast, or average individuals. Freeman, as reported by Hartson (17), indicated that an increase in the extent of a single movement in writing does not necessarily mean increase in duration. A similar finding is also reported by Stetson (36).

Buytendijk (4) reports parallel results in activities somewhat similar to those used in this study. Isserlin, as mentioned by that author, showed that duration of complex movements with the hands and arms is relatively constant within variations in the extent of the trajectory. Derwort has spoken of the "Regel der konstanten Figurzeit," and Buytendijk concludes that tracing numbers is done at a constant speed within variation of size up to three times larger. The same applies to making circles and simple movements of translation. This particular aspect of expressive movements in different tasks should be further explored since it is probably related to the individual's temporal organization.

In another study (24) we have more evidence related to this phenomenon. Certain speeds are strongly preferred by the individual, but the reasons for choosing them, their relation with other performances, and so on, are problems still but slightly explored.

It is not difficult to explain the clustering of tests 30 and 31 with other similar motions done with the hands. Allport and Vernon (2) have obtained similar results with these two tests in relation to speed of drawing with the

hands; but, while their table shows high correlations between speed of copying prose paragraphs and writing *e*'s, with drawing with the feet, our table of intercorrelations shows only a slight average positive coefficient of .24 between writing *e*'s and drawing with the feet, and a nearly zero average correlation for copying paragraphs and drawing with the feet.

Allport and Vernon's study and the present one provide enough evidence to suggest an exploration of the principal variables involved in handwriting, by using elementary and objective tests. Downey (8, 9) has insisted on the importance of speed in writing as a fundamental variable.

Tests 1 and 2 show a saturation of —.22 in factor H. We do not see any reason why these performances should have negative loadings. One possible explanation could be related to the fact that these two tests were the first administered, and many a subject showed a certain amount of surprise when being asked to perform these activities, which in a way are so different from what is usually requested in testing situations. This interpretation may be too naïve, but we have been unable to find a more plausible one. The negative loading of test 42 is also difficult to explain, especially since the other PMA's do not have negative loadings in any of the factors.

It seems proper in view of the previous description to interpret factor H as speed of drawing with the hands, handwriting included, while factor E could be considered as speed of drawing with the feet.

Factor C

		Factor C
Test 36	Reading news	.67
Test 35	Reading science	.66
Test 34	Reading literature	.66
Test 23	Marbles left	.52
Test 39	Verbal meaning	.50
Test 22	Marbles right	.49
Test 19	Sorting cards	.48
Test 41	Reasoning	.34
Test 42	Number	.47
Test 33	Number of things	.30
Test 48	Recognition	.27
Test 56	Left hand circles	.25
Test 32	Cancellation	.24
Test 57	Left hand squares	.23
Test 38	Speed of walking	.20

An inspection of the loadings in factor C indicates its rather complex nature. Many of the tests involved imply the use of verbal ability in one way or another. As expected, spontaneous speed of reading material of varied difficulty may be explained in terms of only one factor. The tests employed covered prose of different degrees of appeal. In spite of personal preferences and familiarity, individuals tend to be uniformly fast or slow readers, at least within the scope of our experiment. The fact that tests 35 and 36 also show a small saturation in factor B is a matter of some interest, and the relation between tapping speed and speed of reading probably deserves further consideration. Our intercorrelations indicate an average *r* between tests 9 to 16 with tests 34 to 36, of .36.

Three of the PMA's—verbal meaning, reasoning, and number—show high loadings in this factor. For the solution of these tests the ability to read printed material is required. Sherman (38), in Thurstone's *Factorial Study of Perception,* reports for fast readers a superior ability in the verbal factor, in number, and in reasoning. In close parallel to our results she does not find any relation between speed in reading and the solution of the space test. The word fluency test does not require reading to any great extent. If "reading is primarily a perceptual function in which the subject has to make associations quickly with rapidly changing stimuli," as reported in the study mentioned, these visual stimuli are practically absent in the solution of the word fluency test.

Test 19 is also highly saturated in this factor. The performance of such an activity requires the perception of the particular drawing on each card and its inclusion in one of the categories. Here again we find perceptual ability similar to that involved in reading.

Test 33 required the subject to name any object he could see in a particular picture. Those who scored high in this test are those who read fast, sort cards fast, and rapidly solve the problems involved in the verbal meaning, reasoning, and number tests.

The activity involved in factor C seems to be related to the speed with which the subjects perceive visually presented material, and,

consequently, it is not strange to find test 48 also loaded on this factor.

Neither the test involving speed of writing nor speed of counting is loaded in factor C. Allport and Vernon (2) found a verbal speed composite which includes reading, counting, handwriting, and blackboard writing. Our results do not seem to corroborate this point. Our intercorrelations between reading and counting are positive but not high, while sorting cards and number of things are more highly correlated with reading speed than with the test of counting.

Factor C is independent of factors H and E, but is positively correlated with factors F and G. Moreover factor C has small negative correlations with factors A and B.

It seems that to reduce this factor to verbal speed would be to ignore what appears to be a more fundamental function, namely, speed of perception. A factor with the same name was isolated by Thurstone (38) using different tests.

The spontaneous speed of reaction of individuals to perceptual configurations as such is probably a fundamental factor in explaining many characteristics in the personality and the intellectual fields. It is our impression that it has to do with central activities, whether reducible to a physiological explanation or not.

Tests 22 and 23 are for all practical purposes a single test and contribute to define factor D. In view of the nature of these tests, perception is one of the elements involved in their solution.

Test 32 is also loaded in factor C. It is obvious that speed of perception should play a part in the cancellation test. It is more difficult to explain the loadings of tests 56 and 57. Even if an acceptable reason could be found, it would not explain why other kinds of closely related activities are not present in factor C.

Summarizing, factor C indicates a domain pertaining to mental functions and seems to be best represented by what has been called speed of perception. Its importance is obviously great in connection with many aspects of human behavior and its detailed exploration deserves the consideration of those interested in perceptual studies.

Factor G

		Factor G
Test 45	Reaction time to light	.74
Test 46	Discrimination	.68
Test 47	Discrimination and choice	.62
Test 44	Reaction time to sound	.44
Test 17	Ergograph	.27
Test 43	Word fluency	.25
Test 51	Faces	.23
Test 3	Parallel movement arms	.21
Test 4	Symmetrical movement arms	.20
Test 30	Copying	.26

All the reaction time tests cluster in factor G. In tests 44 to 51, *S* was explicitly required to respond as rapidly as possible. While test 46 has no loading in other factors, test 47 has a slight saturation in factor I as well. As will be clearer after studying factor I, factor G seems to indicate simpler kinds of speed performances.

The loadings of tests 43 and 51 in this factor may mean that greater fluency is related to shorter reaction time. Nevertheless, this personality variable is more complex than reaction time and deserves further investigation.

Test 30 shows a loading of .26, but it is also loaded in factors B and H. Test 17 has a loading of .27, but its main saturation is in factor A. If the completion of one movement could be taken as the stimulus for the succeeding one, it would be possible to explain why tests 17, 30, 3, and 4 have some loading in factor G. But if this is true, we can not understand why other tests in our battery are not also loaded in this factor. Although factor G is highly related to reaction time, it represents a more complex cluster than pure reaction time.

Factor G is correlated .24 with factor C. The correlations with the other factors are low or moderately negative. If factor C represents speed of perception, it is understandable why it should correlate with speed of reaction time. While factors A and B are related to motor speed, factors C and G seem to be related to the sensory field, and both correlate negatively with A and B but positively with each other, which is an interesting finding.

Factor I

		Factor I
Test 48	Recognition	.45
Test 50	Free association	.45
Test 49	Equality and difference	.43
Test 40	Space	.43
Test 41	Reasoning	.41
Test 55	Left hand lines	.40
Test 52	Right hand lines	.36
Test 51	Faces	.34
Test 33	Number of things	.28
Test 42	Number	.21
Test 47	Discrimination and choice	.20
Test 18	Counting	.21

Tests 48, 49, 50, and 51 were performed as rapidly as possible. The fact that they cluster indicates some function common to all of them. Apparently this is speed of cognition. It is known that to recognize a figure "as" a circle or "as" a triangle, and to recognize "that" two figures are equal or different requires cognitive ability in the sense of Spearman's laws of noegenesis (21, 25, 26, 34, 35). The fact that test 50 is also loaded in this factor may indicate that the functions required for such a performance are partly determined by the speed of cognitive ability. The same applies to test 51.

The space and the reasoning tests are heavily loaded in this factor, and the number test also has a small loading. We know already by previous studies (37, 40) the nature of the PMA's, and the fact that speed in reasoning and speed in space cluster together is interesting. The solution of these two last tests requires a different activity from the ones involved in the other tests of the PMA's. Verbal meaning and number are not loaded in factor I (number has a loading of .21), but space and reasoning are. Moreover, the first two tests cluster around factor C, where reasoning also shows some loading.

The solution of the reasoning test requires a complex exploration of the possible combinations and relations involved in the items; and the same seems to apply to the space test, where it is also true that some *S*s apparently solve the items in a sort of intuitive way. In this context it is difficult to speak with certainty, since the space factor is one of the more serious puzzles in the field of intellectual endeavor.

The low saturation of test 42 in factor I would indicate the small part that more complex intellectual functions play—at least in a population like ours—in simple arithmetical performances, such as adding.

Test 33 was shown to be loaded in speed of perception and also has saturation in factor I. The activity here required is similar to that involved in recognition of figures, the main difference being that in the first test the subject explores a drawing, naming whatever he recognizes, while in the second the subject has to answer as soon as a particular drawing is recognized.

The loadings of tests 55 and 52 are difficult to explain. The small loading shown for the discrimination and choice test indicates that the successful performance of this test is of a more complex nature than the solution of other reaction time tests.

From the previous considerations it seems that we are entitled to relate factor I to speed of judgment, or perhaps better named, speed of cognition. A similar factor was isolated by Thurstone (38). The existence of a speed of perception, a reaction time speed, and a speed of cognition, as described in the present study, seems to agree with similar factors isolated by others using different tests. These findings can thus be used with a greater degree of confidence to explore many an aspect of personality and human behavior. Factor I is correlated with factors C and G. This agrees with findings reported by Thurstone (38). The negative correlation of factor I with factors A and B would also be in line with the findings obtained for factors C and G, indicating a split in speed of performance for motor versus non-motor activities, a point that will be considered later in this study.

It was found that the scores of the *S*s sometimes indicated greater speed for judgment of equality and differences than for recognition. The final results are: test 48—mean 65.34, S.D. 7.12; and for test 49—mean 59.86, S.D. 8.93. These two values are significantly different. This was interpreted as due to the particular experimental material employed. Accordingly, we devised two shorter tests of ten cards each. One of them with five cards with a square and five cards with a circle, the other set with five cards in which the two

designs were either two circles or two squares, and five cards with a circle and a square. In the first subtest, *S* was asked to name the geometrical figure, and in the second subtest he had to judge if the drawings were equal or different. These two subtests were administered to 47 *Ss*. The results are as follows: The mean for the subtest of recognition is 49.39, with a S.D. of 7.15, and the mean for the subtest of equality and differences is 50.40, with a S.D. of 10.31. The difference between the means is not significant.

These results indicate a drop in the means for both tests 48 and 49 when the experimental design is simplified. This drop is more marked for recognition than for the other test. This might indicate that *Ss* are probably guided by the total configuration of the problem presented rather than by the minute analysis of each of the components. If the latter were the case, we would expect a longer time for judgment of equality and differences than for recognition. Wertheimer (41) and other Gestalt psychologists have reported findings that bear directly on this point. This is also related to the differentiation between the "as" and the "that" types of cognition, as studied by C. Spearman (34, 35), Line (21), and ourselves (25, 26).

Factor D

		Factor D
Test 22	Marbles right	.62
Test 23	Marbles left	.61
Test 20	Finger to nose	.34
Test 37	Pulse	.30
Test 39	Verbal meaning	.27
Test 42	Number	.26
Test 50	Free association	.21
Test 21	Bending body	.20
Test 56	Left hand lines	.20

This factor is essentially a marbles doublet without a clear interpretation. When analyzing factor C, where tests 22 and 23 show a high loading, we discussed the possibility that speed of perception could play a part in the performance of such tests. The fact that tests 20 and 21 show some loading here would indicate that factor D involves motor activity.

It is interesting to find that none of the tests of tapping has any appreciable loading in factor D. Perhaps the fact that we tried to eliminate the influence of accuracy in the performance of tests 22 and 23 explains the particular distribution of some of the motor tests; yet it remains unexplained why other tests do not cluster in this factor.

The correlations between factors A and B with D tend to favor the possibility of interpreting the latter as some kind of motor activity.

Pulse rate appears with a loading of .30. We included this test in the hope of finding some relationship between a physiological measurement and other speed variables.

Factor F

		Factor F
Test 59	Metronome second	.68
Test 58	Metronome first	.65
Test 17	Ergograph	.27
Test 30	Copying	.26
Test 4	Parallel movement arms	.21
Test 43	Word fluency	.21
Test 3	Symmetrical movement arms	.20
Test 49	Equality and difference	.20
Test 55	Left hand lines	−.21

This factor is a metronome doublet. Tests 59 and 58 were included, principally because Frischeisen-Kohler (11) considers that preferred metronome rates, together with tapping, define personal tempo.

We have shown previously (24) the subjective and objective effects that certain metronome rates have on ergographic work. The metronome test could be employed to analyze the interesting field of subjective organization of time. The parallelism between this and physical time is by no means a corroborated fact, and most of the evidence is still on a rather anecdotal level.

Consequently, it was interesting to determine the relation of this test to other objective measures of speed. Is there any relationship between these different measurements? Do personality characteristics affect the subjective experience of time? Rubin (27) has compared some of the ways of approaching the study of time, and Schilder (28) has written about the alteration that this function suffers in pathological cases. Any psychiatrist could report interesting data in this connection. Cooper (7) has reported distortions of the time sense under hypnosis, and his findings point to a lack of agreement between objective and subjective times.

The correlations between metronome rates and the other variables are mostly positive or

zero. Factor F shows positive correlation with all other factors excluding D, E, and H.

THE SECOND ORDER

Four factors were obtained by factorizing the correlations between the primaries, as shown in Table 7. The loadings and communalities are given in Table 8. The matrix of transformation and the final oblique factor matrix are presented in Tables 9 and 10, respectively. The results of the rotations indicate the existence of a non-positive manifold, and a few findings can be established with confidence.

TABLE 8

CENTROID MATRIX FOR THE SECOND ORDER *

	I	II	III	IV	h^2
A	47	47	−27	−14	53
B	56	51	03	−17	60
C	−44	38	49	−18	61
D	55	−04	−17	22	38
E	39	40	24	30	46
F	−30	40	−13	−25	33
G	−28	15	23	28	23
H	25	−15	44	−07	28
I	−48	24	−30	31	47

* The decimal point has been omitted for all entries.

TABLE 9

FINAL MATRIX OF TRANSFORMATION FOR THE SECOND ORDER

	α	β	γ	δ
I	.62	−.43	−.41	−.07
II	.74	.55	.42	.50
III	.05	.35	−.58	.70
IV	.28	−.63	.56	.51

TABLE 10

ROTATED FACTOR MATRIX FOR THE PRIMARIES IN THE SECOND ORDER *

	α	β	γ	δ
A	59	05	08	−06
B	68	16	−13	15
C	−02	68	−04	47
D	36	−46	−02	−07
E	63	−05	02	49
F	03	46	23	00
G	03	11	20	40
H	05	01	−44	18
I	−05	04	65	10

* The decimal point has been omitted for all entries.

In factor α both factors A and B are heavily loaded. Moreover, factors A and B do not have any high loadings in the remaining second order factors. Factors C, F, G, and I, of a conspicuous nonmotor kind, show loadings close to zero in factor α. According to our interpretation of factor E, it is not strange to find it loaded in factor α, and the same applies to factor D. The intercorrelations between the primaries add further evidence on this point. The indication is that speeds of motor activities are related by means of a fundamental motor function basic to all of them.

An interesting point is found in the fact that factor H has no loading in factor α, while the primary factor E is heavily loaded in it. Both are, of course, motor activities, and their tests are positively correlated among themselves, although the correlation between the primaries is not very high. It is obvious that in drawing with the hands the influence of a purely motor function is less important than in drawing with the feet, probably due to the effects of cultural factors.

The intercorrelations between the tests of factors E and H may be due to a different kind of function from the purely muscular one involved in drawing. The tests of factor E are correlated with those of factors A and B, while the tests in factor H have smaller correlations with those in factors A and B. In our case, the effects of a drawing speed composite are less important than the effects of a common factor to all motor speeds.

Factor β is bipolar. Factor C and the metronome doublet appear with the highest loadings. Factor D has a saturation of −.46. Since both factors D and F are essentially doublets, these results cannot be interpreted with much confidence. Factor β could be understood as a kind of fundamental speed activity, independent of motor speed and other nonmotor functions. The loading of factor F in β should be kept in mind for further studies.

Factor γ is also bipolar, with factor I most heavily loaded on it, thus indicating intellectual speed. It is clear that this factor is independent of motor functions and of the perceptual kind of activity represented by fac-

tor β. It is difficult to explain the negative loading for factor H. It may well happen that the speed for highly-trained simple activities interferes with speed in activities of a more creative and complex nature.

Factor δ brings together factors C, E, and G. That speed of perception and reaction time appear together is not surprising. The fact that the other factors are not loaded in δ is also easy to explain; but it is quite difficult, at the present stage of our inquiry, to find an explanation for the loading of factor E. Factor δ seems to be mainly related to speed of reaction time.

As a result of our second order analysis it seems possible to isolate the following variables: α, speed of all motor activities; β, speed of perception; γ, speed of cognition; δ, reaction time.

The significance of these results requires further testing, but their meaning and practical implications are undoubtedly of interest. The existence of fundamental factors of speed of perception, of speed of reaction time, of speed of cognition, and of motor speeds are findings that may have considerable theoretical and practical implications. The fact that in intellectual functions there seems to be a speed component, is also of interest. Very probably we are here dealing with some fundamental psychobiological parameters in the realm of speed.

GRAPHICAL RECORD OF MOVEMENTS

For the purpose of discovering the existence of periods of fluctuation in the motor performances, we kept a record of these activities (tests 1 to 16 inclusive) for 25 of our *Ss*. In none of our graphs could we detect any rhythmic oscillation. All the *Ss* performed the activities required in all the motor tests with a complete regularity. These findings agree with previous results of ours in a similar study (24).

By counting the number of movements performed in successive five-second periods, it was possible to conclude that no variation whatsoever was apparent in different *Ss* and tests. In view of these results, it was decided not to continue recording movements for the rest of our experimental population. For the remainder we simply counted the number of movements within the first 15 seconds and in the last 15 seconds, no difference being found between the two scores.

Only two of our *Ss*, when tapping with the fingers, adopted particular rhythms. On later questioning, these persons reported that they deliberately set themselves to send telegraph messages.

Under conditions such as those here reported, individuals adopt for each activity a particular tempo that is kept constant during the whole performance. This fact is also evident in drawing with the hands and with the feet. This particular temporal activity pattern is probably a very good definition of their tempo for the special performance under consideration. Buytendijk (4), Hartson (17), Brown and Slater-Hammel (3), and others have discussed this problem.

The experimental study of these points is of interest in relation to the organization of psychological activities within a temporal frame. Chweitzer, Laugier and Vahl (5), Ricci (23), Kupke (19), Harding (14), Lewis (20), Guhlstorf (13), Rimoldi (24), and others have studied some of these aspects.

STUDY OF INDIVIDUAL CONSISTENCY

As indicated by Coombs (6), it is possible to obtain a variety of meaningful measurements from a table of scores. In our particular case we examined the measures of central tendency and dispersion for each individual on tests 48, 49, 50, and 51 and on the space, reasoning and number tests of the PMA's. For these nonmotor tasks it was observed that, while certain *Ss* seemed to be quite constant in the time they required for each answer in a particular test, others varied within wider limits, indicating that the smaller the range of variation, the more consistent the individual. But, how are these variations in consistency related to the individual's tempo?

We used the coefficient of variation as a measure of variability and plotted it against speed. A small coefficient of variation and high speed tend to go together, while in slow individuals the variability may be either high or low. The graphs show the points spreading in a fan-like fashion (Fig. 1), suggesting that the relationship is probably a rather complex function. This is repeated with

different degrees of clearness in the plots for tests 40, 41, 42, 44, 50, and 51.

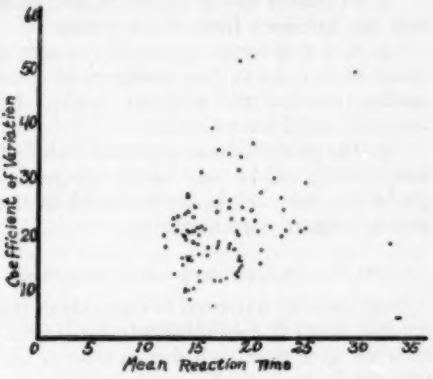

FIG. 1. EACH POINT REPRESENTS AN INDIVIDUAL SUBJECT'S VARIABILITY AND MEAN REACTION TIME TO SOUND

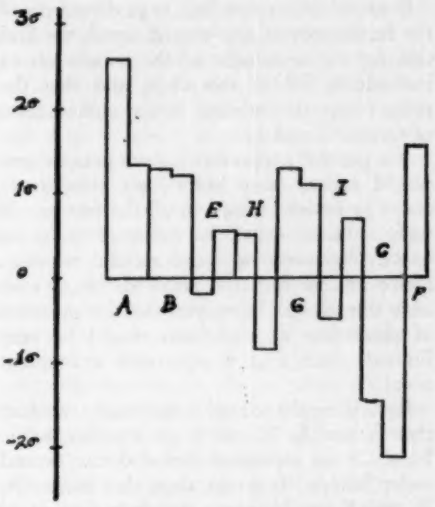

FIG. 2. AN EXAMPLE OF ONE TYPE OF PROFILE FREQUENTLY FOUND AMONG OUR SUBJECTS

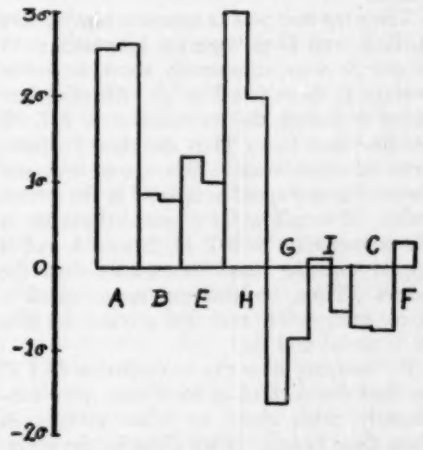

FIG. 3. A PROFILE INDICATING THE TENDENCY OF MOTOR SPEEDS TO CLUSTER TOGETHER

It seems that the speedier the individual, the more consistent—at least for a definite activity. This opinion is advanced in a study by Reymert (22), who, using sound stimuli, reported that the lower the average median reaction time, the lower the mean variation. In our case this relationship seems to extend also to more complex psychological functions, and consistency probably represents a fundamental psychobiological characteristic of the individual.

Subjects within the slow and median ranges of speed are sometimes very variable and other times consistent. For practical purposes it would be advisable to check individual consistency in the slow *Ss* in each particular case. It might be interesting to know which are the causes underlying this relationship, and what are the effects when well-defined personality parameters are kept constant.

THE PROFILES

The raw scores were transformed into standard scores, and for each of the 91 *Ss* the corresponding profiles were plotted, taking into account the two most heavily loaded tests in each factor. (See Figs. 2 and 3.) Inspection of these profiles gives a rapid and graphic representation of each *S*'s relative position in the different factors. Factor D was not used for the construction of these graphs.

Of 91 *Ss*, only three are consistently above or below average in all eight factors. Examining the consistency in any seven factors, we find that only ten individuals are consistently above or below the mean. Factors F and H are the most frequent disturbers.

If we allow for two factors as disturbers of the consistency of the general trend, we find that for the remainder of the population 12 individuals fall in this class, and that the most frequent disturber is the combination of factors G and I.

If a general tempo factor were present, one would expect more individuals consistently above or below average in all the factors. If such a factor exists, its influence must be heavily balanced by more specific activities above and beyond the small common variance due to it. Moreover, for the purposes of prediction its usefulness would be very limited, since Fig. 2 represents a frequent trend.

Reviewing the second order results, we find that factors A, B, and E go together, while I and G are separated into different second order factors. It seems, thus, that factors A, B, and E would give a rough indication of the *S*'s tempo functions provided due consideration is taken of the influence of factors G and I.

The next step was to examine how factors A, B, E, and H go together, inasmuch as 28 of our *S*s were consistently above or below average in them (see Fig. 3). Allowing one factor to disturb the constellation A B E H, we find that factor H is the most frequent cause of disturbance. This agrees with our factor of motor speed as isolated in the second order. If we allow for a second disturber in the constellation A B E H, factors A and B appear together more consistently than the others. Thus, for defining motor speed it seems quite safe to take into account the tests of factors A and B.

By studying now the constellation G I C we find that a third of our *S*s are quite consistently either above or below average in these three factors. If we allow for the agreement of only two of these three factors, we find that for the remainder of our population the combination of factors G I and C I are the most frequent; that is, speed of cognition and reaction time, and speed of perception and speed of cognition, respectively.

Summarizing, we conclude that:

1. It is not possible on the bases of one or two isolated speeds to predict speed in other psychological functions.

2. Motor speeds are best predicted by means of the tests involved in factors A and B and, to a certain extent, in factor E.

3. Nonmotor speeds cannot be predicted with any assurance from motor speeds.

4. As a very rough approximation motor speeds seem to be the best indicators of other speeds, provided the activities implied by factors G and I are excluded.

5. The postulation of a general factor of tempo that could be used for the purpose of prediction, seems to be unwarranted in the present stage of our knowledge.

SUMMARY AND DISCUSSION OF CONCLUSIONS

Since we were interested in the study of the natural tempo of the individuals, the instructions we gave were such that in most of the tests the *S*s were asked to work in their most natural, congenial way.

The variety of tests employed covers a relatively large field of different psychobiological functions. If all these functions could be explained in terms of one single factor, this result would give strong support to the monistic viewpoint.

Among those who have supported such a viewpoint Frischeisen-Kohler (11) stands foremost. This author employed preferred rates of tapping and metronone as her tests. The same variables plus others were used in the present study, but the experimental findings do not corroborate Frischeisen-Kohler's opinion. Since we have dealt quite at length with these points in other parts of the paper, we shall only say that it is safer to accept the existence of different tempos than to accept a single, universal one. It might well happen that by using a small battery of tests, one could reach a different conclusion in view of the fact that the variables might be positively correlated. But this does not mean necessarily unit rank. Moreover, our table of correlations indicates clear clusters.

For instance, we cannot predict from the speed in tests like metronome, the speed of the same subject in bending the body. We know that if the subject has to perform an activity where speed in bending the body is one of the crucial elements involved, a test like swinging the arm is a better indicator than, let us say, drawing with the hands, or a reaction time test, or preferred metronome rate. It would be dangerous to draw con-

clusions from one or two tests on the assumption that these are good indicators of a general tempo.

However, we do not accept the extreme piecemeal viewpoint, according to which tempos in different activities are totally unrelated. The multiple factor view has been defended by other authors. Verbal, drawing, and rhythmic speeds have been mentioned by Allport and Vernon (2). We discussed this previously and found slightly different kinds of factors, which could be explained in terms of the composition of the batteries used. The verbal speed factor of these authors is related to what we called speed of perception and their rhythm factor is related to speed of small movements.

A review of the references indicates that speed for large movements, reaction time speed, and speed of cognition have been isolated in several studies. We believe that all these and the previously mentioned findings can thus be included in our psychological knowledge as relatively well-identified parameters.

But is there any relation between these different speeds? We have shown that all the motor activities cluster together (A, B, and E factors), and a more fundamental function was thus indicated. This means that it seems possible to accept the existence of a motor speed of quite a general kind. The profiles of our subjects also seem to support this opinion, but any prediction based on these terms should necessarily be of a conservative kind. Thus, while it is relatively safe to predict from speed of large movements to speed of small movements and vice versa, it is less safe to predict from these two to speed of drawing with the feet, and practically impossible to predict from these two (that is, speed of large and speed of small movements) to speed of cognition and speed of reaction time.

Speed of cognition, speed of perception, and speed of reaction time are quite well identified in this and in other studies. Thurstone (38) has isolated similar parameters. In this study the author suggests the possibility of a speed component independent of the intellective factors. It is very probable that this component is our speed of cognition, thus indicating the influence of speed as independent of cognitive ability in intellectual performances. Slater (33) has advanced, in a somewhat related study, a similar opinion.

The way in which the PMA's split in our study indicates that the reasoning and space tests require speed of cognition, while the number and verbal meaning tests are more closely related to speed of perception. These results may have interest for explaining intellectual performances and for devising measurements where these particular points are duly considered. For instance, slowness can be as serious a handicap in the solution of a mental test as lack of the ability required for its solution.

Similar reasoning could be advanced with reference to speed of perception. Probably reading difficulties and the differentiation between slow and fast readers have to do with this kind of ability. It would be interesting to know how speed of perception is related to physiological variables like visual acuity and so on, and if this factor is also operative in other senses. If this is the case, speed of perception would indicate a central factor probably of great influence in behavioral patterns.

Although speed functions are better understood in terms of several clusters, it may be possible to obtain rapid and rough information about an individual's tempos by exploring speed in motor performances, speed of perception, and speed of cognition. The tests for this purpose could be easily selected and improved by exploring our tables.

Another point to examine is the individual's variability; that is, are *S*s consistent in speed functions? Our results indicate that fast individuals are consistently fast and slow individuals consistently slow within relatively long periods of time (from two to four weeks), just as Frischeisen-Kohler (11) concludes that time "between sessions has an inconsiderable influence on the variability of tempo." The relation between speed and variability has been analyzed in previous pages and, as far as our experience goes, it seems that this relation is limited to certain specific functions. We tried to find out if those individuals with small variation or greater consistency in some kind of reaction time were also those whose profiles showed a greater evenness. Thus far, our exploration

has failed to give any clear results, and no general conclusions can be drawn.

Another point of interest is the study of the regularity of different performances. The results of previous studies seem to agree with our finding that within certain limited periods of time, performances are carried on at a constant rate. We have been unable to detect any variation in the temporal sequence of events during periods of different length, which indicates that certain activities are performed in an even, regular way. We found similar results in the case of ergographic work (24). Each individual performs a particular act following a certain specific temporal pattern which he keeps constant. This pattern seems to be the most economical for the individual, since others externally imposed usually have a detrimental effect. If the problem is envisaged from the point of view of energy expenditure, these temporal patterns are of interest in explaining the characteristics of certain psychobiological systems.

The study of how these temporal patterns are developed, of their variability and resistance to different disturbers and their relation to personality integration deserves careful consideration. They might play a part of unsuspected importance in the behavior of the individual in relation to himself and to the environment. A study of the field should include the exploration of the more subjective aspects of the time experience. Whether objective time is the correct unit for the evaluation of psychological activities is a problem that still requires exploration, but at present, we have to rely on this unit until a psychological unit is defined.

Summarizing, we feel entitled to conclude that the monistic view of personal tempo does not seem to agree with the experimental findings, and that the practice of determining an individual's tempo by means of one or several tests of tapping is unsatisfactory. If a general tempo factor exists, its influence is very limited due to the existence of definite clusters of speed as described in previous pages.

Individuals seem to be constant through long periods of time in their tempo characteristics. Each *S* seems to adopt a certain temporal pattern for a particular group of activities, and this is the best definition of his "personal tempo."

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PERCEPTUAL ASPECTS OF REPRESSION[1]

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In spite of Freud's many contributions to clinical psychology, psychoanalysis has remained largely subjective. Its basic postulates have defied objective definition. This criticism is especially applicable to the concept of repression, which must be considered one of the several cardinal concepts of psychoanalysis. Without it the structure of the theory breaks down. For this reason more time and effort have been spent in dealing with repression than with any other psychoanalytic concept.

Definition of Repression

Freud has discussed two kinds of repression (2). *Primal repression,* which is the more basic, is the denial of entry into consciousness, to the mental or ideational presentation of an instinct. In addition, "it is a condition of repression that the element of avoiding pain shall have acquired more strength than the pleasure of gratification" (2, p. 85). *After-expulsion,* or repression proper, is the drawing into unconsciousness, as well as a pushing into unconsciousness from consciousness, of material associated with primal repressions. This material may or may not be intrinsically painful. The material in question is thought to conflict with the socially imposed demand for restriction and rejection, i.e., the superego. This conflict results in conscious ego anxiety which motivates the repression. Repressions are maintained through "anticathexes" or countercharges, produced by the ego to meet the excitation of the instinctual impulse. The interaction of these forces produces symptoms and other transformations in behavior. The essential nature of repression for psychoanalysis becomes clear with the realization that it is at the basis of all neuroses and all adult character dispositions.

[1] This experiment was conducted at the University of California, with Professor R. N. Sanford serving as faculty adviser. The author is indebted to him for his many valuable suggestions and criticisms.

Previous Investigations

General surveys of the experimental work in this area have been performed by Sears (9) and Rapaport (8) and will not be repeated here. The reader is referred to these sources for complete summaries and discussions. Certain criticisms of this experimental work which are especially pertinent to the present paper will be dealt with at some length.

Most of the previous work was done with material that was consciously pleasant or unpleasant to the *S*s. However, it has been shown by Sears (9) and Rapaport (8) that these experiments are largely irrelevant to the Freudian theory of repression, which actually assumes *unconscious* processes. It seems evident that, while the results of such experiments may add to memory theory in general, they certainly bear little relation to psychoanalytic repression, the testing of which is their avowed purpose.

A basic methodological objection to most of the experiments on memory for pleasant and unpleasant material concerns the complexity of the test material employed. It is possible that the use of simple words in these experiments precludes the possibility of obtaining relevant (though not necessarily positive) results. It is questionable whether any single words can be repressed, no matter what their presumed meeting might be. It does not seem likely that the ideational content of a wish is generally given by a single word. Moreover, the opportunity for the misinterpretation of single words is great. The word *love* may be given a sexual connotation by some people and not by others. Indeed, it is not far-fetched to expect that certain people, especially children, will consider the concept *love* without a human referent, as in the case of loving candy or the movies. It is also known that some process of intellectualization, instead of after-expulsion, may take place. It seems reasonable to expect that intellectualization is more

easily accomplished with single words than with more fully expounded concepts. These considerations lead to the notion that the design of experiments should insure that the experimental variable is actually manifest. It would appear the pleasant, unpleasant, and neutral words have been used because they could be invented and administered easily and quickly, and because they could be interpreted objectively. Certainly the quest for objectivity as well as economy of time and effort in experimentation is necessary, but this should not be at the expense of the experimental variables. An experiment is designed to test specific hypotheses, and the very first consideration must be the determination of the degree to which the experimental design is capable of testing these hypotheses. We have heard the caution against shooting sparrows with cannon balls. We must guard equally well against shooting bears with BB guns.

A second group of studies has dealt with the memory for life experiences. These are more relevant to the Freudian theory because they concern experiences more likely to be repressed. But, here again, serious criticisms have been offered. Especially important is Rapaport's discussion (8, pp. 69–70). It is questionable whether truly painful experiences are communicated to the *E* even if remembered. This would be true especially of students reporting experiences to their teachers, and many of these studies have utilized the classroom. Moreover, those studies which obtain reports of memories from the same *S*s at different times commit a serious methodological error, namely, there is no way of determining whether the *S*s are remembering the original experiences, or their subsequent reports of them, or both (10, p. 990). Thus, if *S* remembers 70 per cent pleasant experiences on the occasion of a first recall, there is good reason to expect the proportion to increase with time, for he now has had experience (in recall) with more than twice the number of pleasant concepts.

With very few exceptions, all of the studies in the field have been of these types. Most of them are subject to one or more of the criticisms which are given above. On the basis of these studies it is fairly well established that, in general, memory is better for pleasant words and experiences than for unpleasant ones, but the relation of these findings to psychoanalytic theory cannot be evaluated.

LOGICAL CRITICISMS OF REPRESSION

Unconscious Perception

Let us re-examine the process of repression as stated by Freud. In the definition itself there is seen to be operating an object, or process, or subsystem which has the capacity for unconscious perceptions. Freud gives an analogy to a sentinel at one's door refusing entrance to a forbidden guest (2, p. 91). It would appear that this analogy is closer to the formal statement of repression than is at first evident. For the ego (or the unconscious portion thereof) seems to have all the capacities of the organism of which it is a part. It has the ability to perceive independently of the perceptions of the whole organism. It has the ability to induce selective forgetting independently of the whole organism. It has the capacity to keep material hidden despite the best efforts of the whole organism to grasp it. It is, indeed, a sentinel. Such a conception is tinged with vitalism in that it refers the search for explanation back to the ego or superego which, according to Freud, behave unpredictably (2, p. 89). In this sense the concept fails to explain anything. It increases knowledge little to maintain that repression is carried out by a subsystem which operates according to its own laws which cannot be determined. In order to advance our state of knowledge, the psychology of the sentinel would have to be written out. We would have to study the ego's "mind" to determine its most probable reactions. It seems more practical and far less confusing to study the organism's most probable reactions directly and without recourse to the psychology of an intervening variable or, at best, to the psychology of a hypothetical construct.

Suggested Reformulation

A first approximation to the goal of getting objectivity into psychoanalytic theory may be made in the attempt to link repression to perception. If repression is considered a perceptual process, it can be treated in the same manner as other perceptual processes.

We may distinguish between the internal and external organizing forces in perception (5, pp. 100–102). The internal or autochthonous forces are those existing within a process distribution and operating in the direction of simplest form. Thus, if a black square is projected on a screen under conditions of very low illumination, *S*s will tend to see a circular shape. The external forces exist between the process distribution and a stimulus pattern, e.g., stimulations on the retina. To demonstrate the effects of strong external forces, we need only increase the illumination on the figure. Under these conditions *S*s will tend to see a square. It is assumed here that under certain conditions the field is so structured that the internal forces may most economically structure stimuli into perceptions not dealing with sex, just as they best structure geometrical stimulus patterns into circles. However, the fact remains that under some conditions we do see squares. How, then, can such a conception explain repression? The answer lies in the relative strength of the internal and external forces. If a picture of a penis is presented at various levels of illumination, those *S*s may see it earlier and more clearly who are not under the influence of the particular field conditions mentioned above, namely, specific kinds of repressions. On the other hand, those *S*s who have been punished, let us say, for masturbatory activities may not see it. As the external forces become greater, however, more *S*s will make accurate perceptions.

It may be seen, then, that with relatively strong external forces, there tends to take place a perception which corresponds most nearly to the stimulus distribution. But in the intervals between low and high illumination the kinds of perception made are highly variable. As the external forces increase in intensity, changes in perception take place, with the degree of change being directly related to the relative intensity of the external forces. As the illumination increases, for example, *S* may see, in turn, a canoe, a cigar, a banana. If a similar state of affairs were true in the case of repression we should expect the process to be distributed continuously and not discretely. It does seem that some memories may be partially repressed; certain aspects of them may be repressed while others are not; some memories may be wholly available to the organism while the connection between them is not. In this regard, it is becoming increasingly evident from the analysis of normal persons that the process of overcoming repressions is primarily one of inducing the patient to see relationships which he has not been able to see previously.

Repression is considered here as a particular kind of equilibration process in individuals. In the process, memories may disappear or become distorted in a direction making for most economical equilibration. In primal repression, this disappearance or distortion resolves the conflict between an idea and the anxiety associated with punishment for having that idea. After-expulsion describes the condition in which the perceptual field, having been altered by the preceding state of affairs, changes the situation in a way such that new perceptions and memories relating to those already repressed can only be assimilated or perceived with a great outlay of energy. Roughly, this expenditure of energy will be proportional to the degree of similarity between the new material and repressed memories—enough energy, that is, to overcome the resistance of the field of those memories. The situation may be compared to that of a net which allows pebbles, but not rocks, to pass through. The perceptual field is the net, and the pebbles and rocks are the admissible and inadmissible phenomena. The same situation is exemplified by a magnetic field which attracts metals but not wood. In the structure of these fields the conditions are already given in such a way that the field operates in accordance with the particular tensions in it. If the rock is thrown with enough force, it will break through the net and consequently change the field. But until that degree of force is applied, the net will continue to "reject" the rock.

It need hardly be stated that this partial translation of psychoanalytic concepts into the formulations of Gestalt psychology has been very sketchy. There are many aspects of psychoanalytic fact and theory which cannot be handled by Gestalt psychology at present. It is hoped that in time more and

more aspects can be encompassed. But a beginning his been made.[2] Even at this point steps have been taken toward the elimination of the subjectivity of psychoanalysis. Moreover, it is possible to draw some conclusions in the form of predictions from the paradigm of the perception of figures under various degrees of illumination. Meaningful material may be substituted for the square. If this material is similar to material previously repressed, the following observations should be made.

1. Repression will be manifested in perception.

a) Under a sufficiently low degree of illumination repressible material will not be perceived.

b) Under a sufficiently high degree of illumination this material will be perceived by most *Ss*. Some *Ss* may be so affected that the external forces never become great enough to structure the perception in accordance with the stimulus distribution.

c) Between these two stages there will be areas of uncertainty during which the material will be distorted to a greater or lesser degree. The distortions will be of such a nature as to reduce affect, that is, in the direction of the perception and report of material less charged with affect.

d) The threshold for perceiving this material correctly will be greater than that for the perception of material unrelated to previous repressions.[3]

THE EXPERIMENT

Material

Eight sentences were used in the experiment. Four of these served as experimental (E) sentences while the remaining four comprised the control (C) sentences.[4] Each sentence consisted of 14 words and 18 syllables. In addition, the form of every one was the same, that is, the second clause in every case carried the affective meaning, while the first clause served as a modifier. Each E-sentence was matched with a C in terms of the number of meaning units contained in the second clause (see Table 2). This was done to hold constant the structural difficulties of the sentences which influence the threshold of perception.

The experimental sentences described (1) sexual and (2) aggressive impulses directed at one or both of the parents. There were two sentences in each of these two groups. The control material was prepared in such a way as to avoid sexual or aggressive connotations insofar as possible. These sentences were judged by *E* to comprise non-pleasant material. A priori judgments of the repressibility of the experimental material were made since objective criteria were lacking for the evaluation of repressibility of material. It was deemed advisable, therefore, to return to Freud and psychoanalysis for a definition of repressible material. There seemed to be some agreement among psychoanalysts that sexual and aggressive attitudes toward the parents did constitute the primally repressed.

TABLE 1

TEST SENTENCES IN DECREASING ORDER OF DIFFICULTY *

	Control Sentences
1.	I did break some things but I felt that father usually was too stern.
4.	My parents gave me toys and then were angry whenever I played too late.
6.	Whenever I tracked dirt and mud in the house I was scolded by mother.
3.	As I clung helplessly to mother I was taken to school the first day.
	Experimental Sentences
5.	Though mother treated me well I sometimes hoped she would die an awful death.
7.	My parents were in bed and I jealously wanted to kill them for it.
2.	As I saw father asleep I wished terribly to lie there close to him.
8.	When I was young I often had happy dreams of sleeping with my mother.

* The sentence numbers refer to the order in which they were presented at illumination step 1. The sentences are matched 1-3, 4-7, 6-2, 3-8.

Each sentence, including four practice sentences, was photographed and placed upon a 3½ by 4 inch lantern slide to be projected on a screen. The sentences were in white against a black background. A list of the sentences appears in Table 1.

The Subjects

The *Ss* were volunteers from the first and second courses in elementary psychology. They were primarily sophomore and junior students and, in general, knew little about psychoanalysis or about

[2] Such a beginning is seen in the work of Hochberg and Gleitman (4), who are attempting to check experimentally their theoretical derivations of the effects of motivation on the perceptual field. Unlike previous experimenters on autism, Hochberg and Gleitman believe that the laws governing motivational and perceptual phenomena are in fact the same.

[3] Predictions may also be made concerning effects on memory. These predictions, together with experimental data, will be presented in a later paper.

[4] In order to obviate some objections stated previously, sentences were employed as experimental material. Sentences which state specifically certain wishes will be more likely to receive unequivocal interpretation than single words.

psychological experiments. In all, there were 35 female and 22 male *Ss* distributed among three experimental groups.

Procedure

The slides were shown by means of a projector in which the intensity of illumination was varied by changing the resistance within the circuit. As a result of pretest trials with graduate students, seven graded steps of illumination were chosen on the basis that at the lowest illumination no sentence could be seen correctly, while successively more *Ss* were able to see the sentences correctly as the illumination was increased. At the highest level every *S* was able to see every sentence correctly. The attempt was made to space the illumination steps at approximately equal intervals. Hereafter, the illumination steps will be referred to by numbers 1 to 7, indicating the progression from minimum illumination through maximum illumination.

The eight slides used in the experiment were arranged in random order. The final arrangement was that represented by the numbers of the sentences in Table 1. All the slides were presented at step 1, then again at step 2, and so on. In order to distribute practice effects, the order of slides presented at each illumination step was varied randomly with the single exception that no slide was permitted to appear in the same position in the series more than once. There were, then, 56 presentations of experimental material (i.e., eight slides at each of seven steps). In addition, four practice slides were shown to the *Ss* at every illumination step, both to acquaint them with the procedure and to aid them in dark adaptation. Each sentence was exposed for three seconds at every illumination step. The *Ss* were given as much time as they needed between presentations to write what they had seen.

Instructions

The *Ss* entered a fairly dark room, seated themselves, and then waited for from five to ten minutes before receiving the following instructions:

> This is an experiment in perception, as you were told in class. I am going to show you written material on this screen and I want you to write what you have seen on the sheets I am distributing. In each case a slide will be presented for three seconds during which time you should look at it carefully. Following the presentation you will have time to write what you have seen. Now, the form of all the material is the same but the content will vary widely. I should warn you that some subjects have been surprised, shocked or made anxious by some of the material I am going to present. And some of you may be surprised, anxious or shocked. However, don't in any case refrain from writing something because you feel it couldn't mean what you think it says. In those cases it may mean just that. I am going to present these slides at different illuminations. Most of you probably will not be able to see the material at low illuminations. Don't worry about that. Just do the best you can.

The *Ss* were then shown each of the four practice slides at each of the seven steps. The practice was continued until every *S* felt certain that he understood the instructions.

The protocol sheets were divided into thirds by dotted lines. Each third contained eight lines. The *Ss* were instructed to fold the sheets under when they reached the dotted line. Since no slide appeared twice within ten presentations, this measure eliminated the possibility of looking at previous responses and, perhaps, adding to subsequent perceptions. After the practice period but before the presentation of the experimental material the instructions concerning the shocking nature of the material were repeated. These instructions were given in order to help overcome the possibility of obtaining differential results on the basis of suppression of the perception. It seemed that some *Ss* might see the material but refrain from writing it.

ANALYSIS OF DATA

Scoring Problems

Previous work has shown the difficulty of obtaining objective criteria for the scoring of written material. One thing appears certain. In working with the productions of an individual, it is probably better to sacrifice some objectivity rather than subjective significance. The quest for objectivity has meaning only after it has been determined that significant material is actually being investigated. The primary consideration which guided the scoring of *Ss'* responses was the meaning of the material. Each sentence was broken down on the basis of previously established criteria in terms of meaning units. Every E-sentence was matched with a C-sentence for the number of meaning units.[5] The appearance of these units or synonyms for them served to give a perfect score. It is true that this procedure made it possible to overstate or to understate scores of certain *Ss* because of unique manners of expression, but it was hoped that for the group as a whole, the accuracy would be great, as well as considerably more significant than mere scoring for words. The meaning units are given in Table 2.

TABLE 2

MEANING UNITS OF TEST SENTENCES

1. I felt	father was	usually	too	stern
5. I hoped	she die	sometimes	awful	death
4. And (they)	were angry	I played	too late	
7. I wanted	kill them	jealously	for it	
6. I was scolded	by mother			
2. I wished	lie close			
3. I was taken	school			
8. (I) sleeping	with mother			

[5] Only the second clauses were matched for meaning units. It was believed that the attempt to force the entire sentence into a rigid pattern would have introduced several complicating factors not the least of which is artificiality.

The total number of responses in the experiment was 3,192.[6] Each of these responses was given one of five ratings. There were individual ratings for each of the following conditions. The rating was *zero* when a particular response was entirely omitted. The rating was *1* for a response in which one or more words appeared but were insufficient to be scored as correct. A response was given a rating of 2 when the first clause of the sentence was present and essentially correct. A rating of *3* was given when the second clause was present and essentially correct. Finally, responses were rated *4* when the entire sentence appeared and was essentially correct.

In an effort to ascertain the reliability of the scoring system, the protocol sheets were given to a judge for independent rating. This judge was a first-year female graduate student in psychology, who, however, knew nothing of the purpose of the experiment. It seemed that a judge with some psychological training would be better able to evaluate accurately the meaning of *Ss*' responses. She was cautioned to follow the scoring criteria to the letter whenever possible, and it is believed that she actually was able to do so.

A scattergram was made to show the agreement between the ratings of *E* and those of the judge. Zero ratings were not used in this correlation since they would tend to increase it spuriously. No formal Pearsonian coefficient was calculated because the data did not meet its assumptions of normality and linearity. However, a mere inspection of the data showed that in 3,042 comparisons there were only 25 discrepancies. This degree of agreement is very close to a perfect correlation. Moreover, in checking these discrepancies, it was found that more than half were simple clerical errors, the intentions of the raters being the same. Thus, it was possible to obtain an almost complete correspondence of scores while at the same time scoring in terms of the intentions of the *Ss*.

The final scores used were those made by *E*. Each *S* received one score for each sentence. This score was defined as the first illumination step at which he perceived the sentence correctly. However, two levels of accuracy were considered. It was deemed important to determine the point at which the first clause was perceived correctly as well as the point at which the entire sentence was seen. This step was necessary in order to obtain a measure of the structural difficulty of the sentences, that is, their "pure perceptibility." Obviously, the second clauses could not be used for this since they comprised the independent variable. Basically, of course, it is not possible to divide a sentence artificially. To a great extent the difficulty of a clause is determined by the difficulty of the whole sentence. However, with this caution in mind, the first clauses could be used to suggest perceptual difficulty.

A score was given, then, corresponding to the illumination step at which the first clause was seen accurately (Level A). A second score was given corresponding to the illumination step at which the entire sentence was accurately perceived (Level B). To exemplify this procedure let us suppose that *S* saw the first clause correctly at the fourth presentation and the whole sentence at the seventh presentation. He would receive a score of 4 for the first clause (Level A) and a score of 7 for the whole sentence (Level B). When the perception of the first clause did not precede the perception of the last, both scores were the same. In some instances *S* failed to see the sentence even at the last illumination step. In these cases the arbitrary score of 8 was given. This involves no error since no quantitative measures were used in the analysis (see Problems of Analysis).[7]

Problems of Analysis

The scores given were in terms of illumination steps. Although they are referred to as *1*, *2*, *3*, and so on, they are actually ordinal numbers. They refer to positions rather than quantities. Step 2 does not mean that the given illumination to which it refers is equally distant from 1 and from 3. Illumination 2 might actually be at a point 1.74 of the distance between 1 and 3. It can be seen that the data, as given, forbid the use of quantitative statistics. Attempts made at the determination of scale values through the normalization of the data failed.

A method not based on any assumption of equal intervals was devised to analyze the differences between the two groups of sentences. This method is a variation of chi-square, related to the chi-square of changes (6). It assumes only that each succeeding illumination is greater than the preceding ones. If an individual sees a C-sentence at a given point and does not see the corresponding E-sentence until a later step, it follows that the E-sentence required greater illumination in order to be seen. Consequently, the threshold for perception of the E-sentence is greater. In this method the probability of obtaining a given direction of difference is determined and a statement of confidence made. The magnitude of difference is not taken into account since it cannot be determined.

The procedure may be exemplified in the following hypothetical situation. Let us suppose that in a group of 35 *Ss*, 25 have a greater threshold for the perception of a particular E-sentence than for its mate, while 5 show a greater threshold for the C-sentence. Five *Ss* show no difference. If it is assumed that both sentences are drawn from the

[6] Each of 57 *Ss* gave 56 responses.

[7] In the original analysis a measure was interpolated between these two. A score was given for the correct perception of the second clause alone. However, this was discontinued for two reasons. In nearly every case the second clause was not seen in the absence of the first. Thus, this measure usually duplicated Level B. In those few cases where the second clause was seen in the absence of the first it could not always be determined whether the *S* was producing the correct meaning. In sentence 5, for example, *S* might write, "... I sometimes hoped she would die an awful death." It cannot be determined whether "she" refers to mother, and the presumption is that it does not.

same population, it is clear on theoretical grounds, if there is any variability at all, that 15 *Ss* should have a greater threshold for one type while the remaining 15 should have a greater threshold for the other. The five *Ss* who showed no difference do not enter into the calculation of the difference since they do not affect the result. The problem now is the determination of the probability that a deviation of 10 from the expected frequency occurred on a chance basis. The formula used is the familiar one. Chi-square is equal to the sum of the squared deviations of the obtained frequency from the theoretical, divided by the theoretical frequency. The number of degrees of freedom is 1. It should be pointed out once again that no statement of the magnitude of the difference is either assumed or permissible from this method.

RESULTS

It has been suggested that a measure of pure perceptibility of the sentences might be the point at which the first clauses were seen (Level A). Although it was argued that this is not strictly true, an analysis of the threshold for the first clauses should prove instructive.

TABLE 3

THRESHOLD COMPARISONS OF FIRST CLAUSES OF EXPERIMENTAL AND CONTROL SENTENCES

THRESHOLD	SENTENCES				
	1-5	4-7	6-2	3-8	TOTAL *
E greater	33	20	12	19	25
C greater	7	11	34	25	18
E and C equal	17	26	11	13	14
Chi-square (df=1)	16.90	2.61	10.52	0.82	1.14

* To obtain a measure of the group differences, the median scores for C- and E-sentences respectively are calculated for each *S*. These values are then treated as single scores, and the usual chi-square method is applied. The direction of difference is noted for each *S*, and the sum of these for all *Ss* is used in the calculation of the total.

Perceptual Thresholds of First Clauses

The results for Level A (threshold of perception of first clauses) are given in Table 3. It appears that no differences in threshold of perception exist between E- and C-sentences for the entire group of sentences. This suggests that neither group of sentences is structurally more difficult than the other. Although it may not be maintained with certainty, the weight of evidence favors the view that differences in threshold for the entire sentence should be attributed to causes other than the difficulty of the first clauses.

When the individual sentences are compared, however, certain differences appear even at Level A. The comparison of sentences 1 and 5 shows that the first clause of the E-sentence is significantly more difficult to see. This finding might be used as an argument that differences in threshold of perception of the whole sentence may be attributed to the difficulty of the first clause. However, in the comparison between sentences 6 and 2 the reverse result obtains. Here, the first clause of the C-sentence is much more difficult to perceive. Thus, it might be expected that the effects of sentence 5 would be nullified by those of sentence 6. A much better method for equalizing the difficulty of the first clauses will be discussed below.

By way of summary, it appears that there is no differential structural difficulty in the first clauses of E- and C-sentences. The two exceptions to this conclusion operate in opposite directions, and their effects should be nullified.

Perceptual Thresholds of Whole Sentences

Although the first clauses of the sentences show no over-all differences, the thresholds for the complete sentences differ very significantly (Table 4). As a group the E-sentences are significantly more difficult to see than the C-sentences. However, when this difference is broken down into its component elements, it appears that the result is due primarily to the effect of sentence 5, with secondary support from sentences 7 and 8; sentence 2 is not significantly different from C-sentence 6. Moreover, it was previously argued that sentence 5 might show this result because of the greater difficulty of its first clause. It was thought that the effect was likely to be counteracted by sentence 6. Although the latter sentence did depress the total result, it failed to do so to any appreciable degree. For this reason the over-all difference may not be attributed entirely to the experimental variable.

Sex Differences

If the data are analyzed for sex differences, it can be seen that, aside from sentence 5, the only significant differences appear for women

TABLE 4

THRESHOLD COMPARISONS OF EXPERIMENTAL AND CONTROL SENTENCES

COMPARISON SENTENCES	THRESHOLD									χ^2 (df=1)		
	E GREATER			C GREATER			E AND C EQUAL					
	M	F	TOTAL	M	F	TOTAL	M	F	TOTAL	M	F	TOTAL
1-5	14	26	40	0	0	0	8	9	17	12.07	25.00	39.00
4-7	7	15	22	7	6	13	8	14	22	0.00	3.86	2.29
6-2	8	11	19	8	14	22	6	10	16	0.00	0.37	0.22
3-8	11	15	26	6	7	13	5	13	18	1.47	2.91	4.33
Total			32			3			22			19.32

(Table 4). This result is difficult to interpret for the reason stated above, i.e., it cannot be determined whether the result is due to the effects of the independent variable or to other relevant factors.

Lag in Perception

In an effort to shed light on this problem, the data were subjected to an analysis which held constant the effects of the first clauses. The measure used to achieve this effect was termed the lag in perception. It is based on the assumption that the difficulty of any second clause can be measured by the number of additional presentations of the sentence that are necessary to perceive it *after the first clause has been perceived*. For example, suppose that sentences X and Y were both perceived correctly at illumination step 6, while their first clauses were seen at steps 3 and 5, respectively. Clearly the sentences are not of equal difficulty even though correct perception took place at the same illumination step, for three presentations of sentence X were required after the perception of the first clause before the whole sentence could be seen. Only one such presentation of sentence Y was required. Thus, the difficulty, or lag, in sentence X is 3 (that is 6 minus 3), while the lag in sentence Y is 1 (6 minus 5). Sentence X is presumed to be more difficult than sentence Y when the effects of the first clauses are held constant. It can be seen that the lag measure gives a better statement of the difficulty of perception than do the previous measures.

Table 5 shows the comparisons of C- and E-sentences with respect to lag. The chi-square of the difference in lag for the group as a whole is significant at the 1 per cent level of confidence. This indicates that the E-sentences as a group are more difficult to see than the C-sentences. A comparison of individual sentences bears out this result. In the case of every comparison there is a significant difference in the predicted direction. The differences for sentences 1 to 5 and for 4 to 7 are significant at the 5 per cent level of confidence, while the difference in comparison of 3 and 8 is significant at the 2 per cent level. Every E-sentence differs significantly from its C-mate in the predicted direction. Thus, when the difficulty of the first-clause modifiers is held constant by means of this method, the repressible material is much more difficult to see. This difference

TABLE 5

RELATIVE DIFFERENCES IN PERCEPTUAL LAG OF EXPERIMENTAL AND CONTROL SENTENCES *

COMPARISON SENTENCES	THRESHOLD									χ^2 (df=1)		
	E GREATER			C GREATER			E AND C EQUAL					
	M	F	TOTAL	M	F	TOTAL	M	F	TOTAL	M	F	TOTAL
1-5	8	18	26	5	8	13	9	5	14	0.70	3.85	4.33
4-7	9	20	29	8	7	15	5	8	13	0.06	6.25	4.45
6-2	10	14	24	2	8	10	10	12	22	4.08	1.64	5.75
3-8	14	18	32	3	10	13	4	6	10	4.75	2.28	7.95

* See text for an explanation of the lag measure.

cannot be attributed to a greater structural difficulty of the second clauses since that factor was controlled in the matching. The number of words, syllables, and meaning units is equated, and as a consequence the obtained differences must be attributed to the differential content.

While the differential content of the sentences may actually be responsible for the obtained differences, it may be effective for other reasons than repression. It may be argued that (1) the experimental material was perceived as early as the control material but consciously suppressed out of embarrassment, or (2) the experimental material was not perceived as early because of the unusual nature of the concepts. Proponents of the latter argument would hold the view that the same results might have been obtained with any unusual sentence, such as, "When I was young I often had happy dreams of sleeping with the square of the hypotenuse."

Sex Differences

The sex differences in lag of perception help to refute both of these arguments. It will be observed (Table 5) that, although all differences are in the predicted direction, women make the greater contribution to the observed differences between sentences 1 to 5 and 4 to 7, while men contribute most heavily to the differences in the other two comparisons. It is difficult to reconcile these findings with the above objections. To be consistent, the argument would have to be advanced that different concepts are strange to men and women. This argument can hardly be opposed in general, but its application is dubious in this case unless one postulates a process similar to repression. This becomes clear when the content of the sentences in question is considered. Women find it more difficult to see sentences 5 and 7, both of which are primarily aggressive in nature. On the other hand, men show a greater lag on sentences 2 and 8, which are primarily sexual in nature. These differences are extremely clear-cut. It seems unwarranted to postulate a strangeness or suppressive factor in women for aggressive material, and one in men for sexual material.

As a matter of fact, explanations for these findings can be advanced which are consonant with and predictable from psychoanalytic theory. Freud (3, Lect. 33) has argued that the Oedipus complex in boys is based on sexual attitudes. It is repressed and subsequently dissolved because of castration anxiety, with the consequent formation of a strong superego. It would follow, then, that the sexual attitudes would be repressed and that such repression would be maintained for a long time. In girls, on the other hand, castration anxiety or penis envy *prepares the way for the Oedipus complex*. Its appearance is accompanied by the outbreak of long pent-up hostility toward the mother. No driving force for the repression of sexuality is present here, and the development of the superego suffers. However, girls do repress sexuality to some extent and aggression to a much greater extent. The reasons for this are presumably cultural and constitutional. In normal development, and, according to Freud, by constitution, the girl is forced to give up her aggressive attitudes and assume a passive role. Ordinary conventions prohibit her from manifesting hostile impulses. The acceptance of this role leads to the repression of hostility (and the consequent appearance of masochism in women as a feminine trait).

Pre-recognition Results

The prediction was made that the repressible material would be distorted in the direction of reduction of affect, while the nonrepressible material would not undergo such distortion. Unfortunately, too few cases of distortion were observed to permit any definitive statement about this prediction, but there is strong presumptive evidence that it is valid. No calculation of distortion was made unless the interpretation was relatively clear. For example, the following distortion of the third sentence could not be interpreted unequivocally: "As I clung helplessly to mother—taken the alcohol"; neither could the following distortion of the sixth sentence: "Everywhere I looked, dirt and mud." Unquestionably these productions are significant for *S* but in the absence of individual study they must remain uninterpreted.

The distortions which were considered could readily be judged as manifesting either an increase or a decrease in intensity of atti-

tudes contained in the sentences. There was only one case in which a particular attitude expressed was judged as the more intense. The *S*, a woman, distorted control sentence 6 to read, "Whenever I tracked dirt and mud in the house I was slapped by mother." In all probability the substitution of the word "slapped" for "scolded" constitutes an increase in intensity. In general, the distortions were in the direction of reduction of intensity. A common example of this is the following distortion of sentence 8, "When I was young I often had happy dreams of mother." This, of course, omits the key words, "sleeping with." Another kind of distortion of the same type reduces affect by changing the object of the attitude. Thus, the fifth sentence became, "I hoped *I* would die an awful death." Reversal of parents in the sentences constitutes an important distortion of this kind, but this was treated separately. Finally, a third manner of reducing affect is changing the concept. This was exemplified in changing the seventh sentence to, "My parents were *not bad* and I jealously felt—." Here the aggressive element was not seen by *S*, but the sexual element was turned into a more acceptable concept.

Table 6 contains the quantitative data on distortions. Even with the small number of cases available for study it is apparent that the E-sentences are definitely more affected than the C-sentences in terms of the number of reduced affect distortions and in the number of reversals of parent figures. It is difficult to interpret the latter finding, but it is consonant with the other results to assume that *S*s who perform reversals of parents are more able to accept the given attitude with respect to the substituted parent. The data on the individual sentences are too scanty to shed light on the meaning of reversals, especially with respect to the sex differences of *S*s. However, it is clear that the E-sentences are distorted significantly more often than the C-sentences. This may be interpreted in the same way as the results on threshold. In the case of the threshold results, it appeared that previous repressions have so altered the perceptual field that related material subsequently presented can not easily be integrated. The result is that more energy is required for such integration. The distortion results imply repression, and, in addition, seem to indicate the *S*s' oft-discussed search for meaning. Although the presented material cannot be accepted by the field, the stimulus pattern demands some structure. The structure given will depend on the framework of the individual, his needs, habits, attitudes, and so on. It becomes evident that the obtained distortions can be interpreted much in the same manner as the data of projective techniques.

TABLE 6

DISTORTIONS IN PERCEPTION

TYPE OF DISTORTION	MEN		WOMEN		TOTAL		χ^2
	C*	E*	C	E	C	E	
Increase in affect	0	0	1	0	1	0	—
Decrease in affect	1	8	4	11	5	19	9.78
Reversal of parent	0	4	1	4	1	8	4.00

* C and E refer to control and experimental sentences, respectively.

Sex differences are not apparent in these results. Men show a greater proportion of distortions of E-sentences to C-sentences than do women, but the number of cases is so small as to cast doubt on the generality of this finding. However, if the comparison is made with respect to the content of the sentences, the results are similar to those obtained when this was done previously (Table 7). While the distortions of the men are equally divided between sex sentences and aggressive sentences, the women distort aggressive sentences to a much greater degree than they do sex sentences.

DISCUSSION AND CONCLUSIONS

The results seem to fit the psychoanalytic definition of repression quite well. A "denial of entry into consciousness of [certain] ideational contents" is certainly observed.

Most *Ss* see the experimental material only after the external organizing forces become relatively great. It appears that forces exist within the individual which prevent the appearance in consciousness of certain kinds of material, among which are included sexual and aggressive material. This much has been stated by psychoanalysts for many years. But there are specific gains from these results. The psychoanalytic theory is now supported by evidence which is objective enough to be repeated and molar enough to encompass the kinds of data the analysts have been interested in. Moreover, it may now be stated with some assurance that repression is basically, or at least secondarily, perceptual.[8] With this statement the process of repression becomes a construct in that it automatically takes on further characteristics. It must operate within the law of *Praegnanz*; it must follow good continuation, proximity, common fate, and similarity. The postulation of these laws of field organization has led to valuable research in the field of perception. Now the ground is laid for similar research in the psychology of personality.

It seems that repression has a still closer relation to these laws of organization. The Gestalt laws of perception are stated in extremely general terms. We know that material will be perceived in accordance with the principle of *good continuation*. This principle applies in the perception of simple lines; however, its application to a meaningful material is presently of limited value since it is difficult to state just which continuation is good in any given case. The concept of repression offers a method of answering just that question. It states that a *good continuation* in a particular situation will take place in a given direction. The concept, then, seems to represent a particularization of another general principle, that of *Praegnanz*. This relation may be exemplified by a physical concept. Gravity governs the behavior of all objects as Praegnanz governs the course of all perceptions. The behavior of tides is a specific application of gravity, just as repression is of Praegnanz. However, though gravity operates between any two bodies, tides do not. For the latter to exist, the bodies must be of a specific type and in a specific relation to each other. The masses and distances have to be of a given order for tides to operate. In general, for a specific application of gravity to be effective, specific conditions must exist. Similarly, Praegnanz always operates, but the operation of repression depends on the existence of given field conditions. Before the discovery of gravity, the movement of tides was inexplicable. This is not to say that the general law must precede the specific application. Indeed, general laws are formulated out of inductions from a mass of facts. But the fact remains that clear understanding and explanation of data usually wait for the discovery of a general law. This applies even to the data which led to the formulation of the law. The parallel in psychology is close. Repression was formulated at least as early as Praegnanz. Freud even foresaw the Gestalt principle in his principle of economy of mental functioning. But it was not until the specific law of organization was stated more explicitly that the ground was laid for an objective approach to problems of repression. Within the framework of this discussion repression can be located approximately within the levels of personality. The process is genotypic in the sense that it will account for many seemingly unrelated behaviors, while

TABLE 7

SEX DIFFERENCES IN DISTORTIONS

SEX	AGGRESSION SENTENCES			SEX SENTENCES		
	DISTORTIONS	REVERSALS	TOTAL	DISTORTIONS	REVERSALS	TOTAL
Women	9	3	12	2	1	3
Men	4	2	6	4	2	6
Total	13	5	18	6	3	9

[8] The determination of the basic nature of repression must wait for a study of primal repressions.

in itself it cannot be observed directly. However, it is not as genotypic a concept as Praegnanz. The latter process will, in the light of this argument, account for all the data covered by repression and in addition some behavior not accounted for by repression. Such a conception implies that there may be cases in which repression will be inapplicable. Indeed, there may be cases in which even Praegnanz, as presently understood, will be inapplicable, and where some deeper genotype must be sought. While this argument has never been stated in these terms, it has not gone unnoticed. Malinowski (7) showed that the classical Oedipus complex was too specific a formulation to find application to the Trobriand Islanders. He showed that the concept could, however, be applied if the formal aspects rather than the biological were considered. This is an example of the extension of a general explanation to a still deeper level. It may be that data can be brought forward which will be unexplained even by this more general formulation. If this is the case, the quest for deeper explanations will go forward.

SUMMARY

It is suggested that the difficulty of performing objective studies of psychoanalytic concepts is due to the equivocal definitions of these concepts. A possible solution to the problem is to integrate the facts of psychoanalysis and those of a more objectively stated discipline, such as Gestalt psychology of perception.

In an attempt to apply this solution, an experiment is reported. It was hypothesized that repression and perception are intimately related, and that effects of repression could be observed in perception. The experimental material consisted of sentences describing sexual and aggressive attitudes toward parents. It was thought that sentences would be more successful than single words in tapping the truly basic affective attitudes of the *S*s.

Results bear out the major hypothesis. Repressible sentences are more difficult to see than neutral sentences, and they are more frequently distorted. Women repress aggressive material, while men repress sexual material. This finding fulfills psychoanalytic predictions.

These results prove that a relationship exists between repression and perception. It is suggested that a perceptual approach to personality problems is of basic theoretical importance.

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EXPERIMENTAL FRUSTRATION IN A GROUP TEST SITUATION

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THE present paper describes the culmination of a series of studies designed to disturb human beings emotionally in a controlled but realistic situation. The developed instrument is an easily administered pencil and paper test used in a situation in which the *S* performs first at his own rate and then in a situation designed to be frustrating. In the latter situation, *S*'s performance is continually interrupted, and he is continually told that he has failed to meet the average of his group. Changes in speed and errors under experimental frustration are regarded as indications of degrees of frustration proneness or tolerance in this situation.[1] An attempt has been made to relate this behavior to other measures of the stability of personality.

Whether this behavior indicates general frustration proneness or tolerance as discussed in the theoretical literature (3, 12, 13, 16), or various specific adjustive reactions by different individuals to interruption and derogation of this kind of performance is a matter for further investigation.[2]

The earlier studies on the technique include the following. Strother (15) used a buzzer to signify failure to meet a standard along with derogatory remarks as a means of frustration in individual sessions. He found, as did McKinney (9), Dashiell (4), and Gandine-Stanton (7), that individuals under experimental frustration increased the speed of their responses while sacrificing accuracy. He found only slight inconclusive tendencies for those *Ss* who were more "neurotic" on items from the Thurstone Personality Schedule to be more disturbed by experimental frustration than those least "neurotic." These more neurotic *Ss* showed significantly more negative defense mechanisms which were attempts to justify their performance or which tended to take them out of the field.

Hines (8) used mental multiplication in a similar, but apparently less disturbing frustration design, and had *Ss* report their answer by placing a stylus in designated holes in a steadiness tester. Some *Ss* improved in error scores under the experimental conditions; others showed a decrement. The groups showing high error and low error scores under frustration did not differ significantly in the following items of personality: history or contemporary activities, spontaneous verbal or motor responses during the experiment, and attitudes toward themselves and parents. Experimentally frustrated and control *Ss* differed slightly when all verbal and motor responses in the individual frustrating situation were considered as a total basis for classification.

METHOD

One hundred and ninety-six naval radar operator trainees served as *Ss* during World War II. The experiment was performed in a regular class room. The experimenter was introduced by one of the popular officer-instructors. The attitude and behavior of the *Ss* during and after the experiment indicated that they took the experiment seriously and were motivated to do well in it.

The material used was contained in a "Reporting Test," a booklet of 315 small, circular diagrams each of which contained varying numbers of three different symbols. A sample of the problem is shown in Figure 1. Symbols were designated Plane (*Pl*), Ship (*Sh*), and Land (*La*). The *Ss* were asked to

[1] Part of this study was designed in connection with Project SC-70-NS-146: Selection and Training of Radar Operators, Contract OEMSR 919 Contractor: Yerkes Laboratories of Primate Biology, Applied Psychology Panel NDRC, and directed by D. B. Lindsley. Acknowledgment is made of assistance from members of the project and the Applied Psychology Panel. The major experiment described herein was originally published as Informal Memorandum No. 28 (Sept. 10, 1945) to the Applied Psychology Panel NDRC.

[2] Since these experiments were initiated there have appeared in the literature other reports of methods used to frustrate human beings experimentally (2, 5, 6, 10, 14).

count the number of each kind of symbol in each diagram and to record their count on an accompanying answer blank.

These particular stimuli were used because the test was originally designed to be used with trainees in radar operation. Some of the processes involved in perceiving and reporting the number of each kind of figure are similar to those of reading a radar screen during combat. Some trainees were known to become upset emotionally and inefficient under stress, and this test was designed to study such behavior.

The form which the test material assumed was also dictated in part by certain experimental requirements. A test intended to measure emotional disorganization should not be influenced too greatly by differences in intelligence or special aptitude. The material should be easily learned so that the *S*'s performance runs smoothly before the frustrating conditions are applied. In short, the *S*'s behavior under stress should reflect his susceptibility to emotional disorganization when thwarted, rather than any other factors in his make-up.

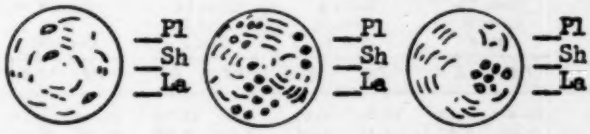

FIG. 1. A SAMPLE OF THE CIRCULAR DIAGRAMS USED IN THE PROBLEM

The booklet of circles was divided into four parts to be used in three experimental conditions. In *Condition I*, the control situation, *S* counted and listed the number of each of the three kinds of figure in each circle, working at his own pace under the conditions of a typical school test or exercise. This period was eight minutes in length. After brief instructions, *S* continued with Condition II. In *Condition II*, the experimental or frustrating situation *S* was told to proceed with the problems in the same manner as in the first condition, but now he would be instructed every 30 seconds on which problem he should be working. If he were not on the problem designated he was to mark "F," for "failed," on the answer blank for each problem not completed, then proceed to the next problem as called by the examiner. The announced level of achievement was beyond the capacity of any subject. Condition II consisted of two sections of eight minutes each with no time interval separating them. *Condition III* was like Condition I in that *S* was allowed to go at his own rate with no instructions as to what he should accomplish.[3]

A mimeographed questionnaire was given each *S* after the experiment was over. It was a check list of feelings and verbalizations which students had expressed during the Hines study (1). The *S* checked the statements which ran through his mind during and after the experiment and handed in the questionnaire before leaving the room.

RESULTS

Effect of Frustrating Condition on Errors and Speed of Work

The first question that arises is: How has the frustrating condition affected the number of errors and the speed of work of the *S*s as a group? The results of this type of analysis are shown in Tables 1 and 2. Table 1 shows speed scores (*PA*, or number of problems attempted) and the average errors (*E*) for the three conditions: *Condition I* was the

[3] A lithoprinted booklet containing the instructions and the key may be obtained from the senior author, or as Document 3209 from the American Documentation Institute, 1719 N St., N.W., Washington 6, D.C., remitting $1.00 for microfilm (images 1 in. high on standard 35 mm. film) or $2.70 for photocopies (6 x 8 in.) readable without optical aid.

TABLE 1

MEANS AND STANDARD DEVIATIONS OF SCORES FOR PROBLEMS ATTEMPTED (*PA*) AND ERRORS (*E*) FOR THE THREE EXPERIMENTAL CONDITIONS

(N=196)

	Cond. I		Cond. II				Cond. III	
			Sec. 1		Sec. 2			
	PA	*E*	*PA*	*E*	*PA*	*E*	*PA*	*E*
Mean	62.52	8.05	76.92	19.20	66.12	20.59	82.01	16.26
SD	14.83	5.48	22.80	15.27	29.03	21.76	17.76	13.17

initial situation of normal work; *Condition II* (containing two equal sections) was the experimentally frustrating period, during which time *S* was led to believe he was failing to meet the standard required of him; and *Condition III* was another period like Condition I in which performance was not interrupted and no standard was required. On the second line of Table 1 is the standard deviation of the distribution of scores.

Table 2 shows the differences in speed of work (*PA*) and errors (*E*) for the three conditions and the reliability of these differences in terms of their *t* ratios.

Speed. The number of problems attempted (*PA*) under the condition of experimental frustration increased over that of Condition I. However, only the difference between Sec. 1 of Condition II and Condition I is statistically significant. It is under Condition III, however, that the number of problems attempted reaches the highest level.

In summary, it may be stated that although the frustrating condition increased the number of problems attempted, it also increased the number of errors. Variability of performance also increased under frustration, indicating that the frustrating condition affected

TABLE 2

DIFFERENCES BETWEEN PERFORMANCE OF SUBJECTS WORKING UNDER THE THREE SETS OF CONDITIONS AND THEIR SIGNIFICANCE

	Cond. I & Cond. II, Sec. 1		Cond. II, Sec. 1 & Cond. II, Sec. 2		Cond. I & Cond. II, Sec. 2		Cond. I & Cond. III		Cond. II, Sec. 1 & Cond. III	
	PA	*E*	*PA*	*E*	*PA*	*E*	*PA*	*E*	*PA*	*E*
Mean Diff.	14.40	11.15	10.80	1.39	3.60	12.54	19.49	8.21	5.09	2.94
t	7.66	9.61	4.11	0.71	1.54	7.65	11.88	8.21	2.47	2.04
Diff. between *SD*	7.97	9.79	6.23	6.49	14.20	16.28	2.93	7.69	5.04	2.10
t	5.83	11.90	3.33	4.81	8.65	14.46	2.50	10.05	3.51	2.20

Examination of Table 1 for differences and of Table 2 for the statistical significance of these differences shows the following trends.

Errors. The errors under the frustrating situation (Condition II, Sec. 1 and Sec. 2) are more than double those under the non-stress condition (Condition I). These differences are statistically significant at an acceptable level of confidence. Although the external stress has been removed in Condition III, the errors are still twice as large as those of Condition I. This may be due to persistence of the effects of experimental frustration speed-stress induced in Condition II, but requires further experimentation for proof.

In addition to the increase in errors under frustration (Condition II), there is a marked increase in the spread of error scores as shown by the standard deviations. The differences between the standard deviations for Conditions I and II are statistically reliable. This suggests that individuals react differently to experimental frustration. This concept will be examined further under individual results.

different *S*s differently. The speed effect obtained during frustration was also present after external conditions of frustration were removed.

Individual Differences in Errors and Speed

It was suggested above that the increase in the variability under the frustration condition might mean that individuals are affected differently by it. The character and extent of these differences are examined below.

Errors. Individual records show that of the 196 *S*s, 112 (57 per cent) had over a twofold increase in error score under the frustration conditions. Of these, 60 (31 per cent of the total) persisted in high error scores (at least a 25 per cent increase over Condition I) during the control situation (Condition III). In view of the number of *S*s who made many more errors under frustration, and in view of the average increase in errors of the group as a whole, an increase in errors might be considered a normal or typical response to the type of frustration used in this experiment.

Twenty-four (12 per cent) were negligibly affected by the experimental situation (less than 10 per cent increase in errors). The performance of these *Ss* remained relatively stable, and there was no loss of efficiency throughout the task. This will be discussed further in the following section.

Speed. In terms of number of problems attempted individual records show that 87 *Ss* (44 per cent) were not greatly affected (less than 10 per cent increase) by the experimental frustration. Fifty-eight (30 per cent) showed an increase of 25 per cent or more. During the final control situation (Condition III) 76 subjects (39 per cent) had at least a 25 per cent increase over Condition II, and 43 subjects (22 per cent) showed an increase of less than 25 per cent over Condition I.

Eighteen per cent of the *Ss* had increases in errors during frustration even though they did not go faster. Therefore, these errors cannot be attributed to increased speed at the expense of accuracy. These *Ss* depart from the trend of the group as a whole, which showed increases in both speed and errors under frustration.

The tendency for errors and speed to increase under frustration and to persist afterwards *in the case of some Ss and not in others,* indicates the importance of recognizing individual differences in efficiency under such a condition. These results show the danger of treating all *Ss* as a homogeneous group in their reaction to frustration conditions.

Effect of Frustration on Efficiency: Combined Speed-Error Scores

Although it is important to know the effect of frustration upon both speed and errors independently, neither score alone portrays total individual efficiency. The over-all efficiency of an individual may be indicated by: (1) the net number of problems solved (problems attempted minus errors), or (2) the per cent correct (number of problems correct divided by the number attempted). The first score emphasizes the accuracy of *S*. The second score indicates the relative efficiency of *S* in terms of his total efforts. An individual who attempts many problems and solves only half of them correctly is relatively inefficient, even though his net scores may be equal to those of a man who attempts many less problems and makes very few errors. If errors represent loss of material or a jeopardy to human safety, the first individual is a liability. If, on the other hand, errors are not expensive, or if a rough estimate is as good as a precise one, an individual who turns out more net observations by working more rapidly is valuable even though he makes more errors. However, errors are rarely inexpensive, particularly if they are gross errors. Table 3 presents combined speed-error scores by these two efficiency measures for two ex-

TABLE 3

MEANS AND STANDARD ERRORS OF MEANS FOR COMBINED SPEED-ERROR SCORES: NET CORRECT SCORE AND PER CENT CORRECT SCORES

	Cond. I		Cond. II				Cond. III	
			Sec. 1		Sec. 2			
	Net Correct	% Correct	Net Correct	% Correct	Net Correct	% Correct	Net Correct	% Correct
Group I								
Mean	52.12	78	51.58	78	42.25	79	62.95	80
σ_M	3.23	2.52	3.53	1.96	2.94	2.56	3.39	2.54
Group II								
Mean	52.55	83	51.83	61	39.77	55	62.13	71
σ_M	1.92	2.25	1.98	1.96	2.60	2.41	2.74	2.62
Group III								
Mean	54.47	87	57.72	75	45.53	75	65.75	80

Group I (N=24): *Ss* with negligible error increase under experimental frustration (stable).
Group II (N=37): *Ss* with greatest error increases under experimental frustration (labile).
Group III (N=196): All *Ss*.

treme groups selected from the total number studied. Group I consists of 24 *Ss* who showed a negligible increase in errors (less than 10 per cent increase) under Experimental Condition II, who might be called stable. Group II consists of 37 *Ss* who showed the greatest increase in errors under stress (more than 25 per cent increase), the more labile individuals. In addition, there are in Table 3 the average speed-error scores of all the *Ss* (Group III).

Comparison of the mean *net correct scores* (problems attempted minus errors) for Groups I and II in Table 3 shows that under both experimental and control conditions the scores are almost equal. There are no statistically significant differences between these two groups in terms of *net correct scores.* However, in terms of relative efficiency shown by *per cent correct scores,* the differences between the two groups under experimental frustration are statistically significant (e.g., the *t* ratio of the difference for Condition II, Sec. 1, is 4.90; $P<.001$). Under Condition I, the relative efficiency of Group II actually exceeded that of Group I, although the difference ($t=1.48$; $P>.10<.2$) does not meet the criterion of statistical significance. The difference in relative efficiency under Condition III was in the same direction as that under experimental Condition II ($t=2.47$; $P<.02$). It is possible that in the case of Group II (*Ss* with greatest error), there was a tendency for the frustration effect to persist into Condition III. In summary, the efficiency of Group II dropped under frustration. Despite a loss of efficiency under experimental frustration, Group II completed about as many problems correctly (net correct scores) as did Group I (*Ss* with negligible error), but made more errors.

The ratio of total problems attempted to those correct, which appears to be the most promising measure of efficiency, shows stability of performance throughout for Group I, but loss of efficiency under experimental frustration for Group II and for the total number (Group III).

Questionnaire Responses and Efficiency Scores

Differential items in the questionnaire that *S* filled out after the experiment were classified into two categories: "Admission of Disturbance," and "Enjoyment or No Unusual Disturbance." The percentage of the *Ss* in the group with negligible errors during experimental frustration, and those with great increases (twofold) in errors ($N=112$) are given in Table 4.

None of the differences between the two efficiency groups in questionnaire responses was significant. However, it should be noted that there is a trend for a somewhat higher percentage of the *Ss*, those with negligible errors, to admit disturbance in terms of the questions they checked. The percentage of *Ss* admitting disturbance was greater in 11 out of 12 questionnaire items showing differences between the groups. The percentage of *Ss* admitting enjoyment of the experiment or no unusual disturbance was greater in six out of eight questionnaire items, for those *Ss*, with greater error during frustration. Although these percentage differences are not significant from a statistical standpoint, this consistency of small differences cannot be ignored. Either the more efficient persons experienced more subjective disturbance, or the less efficient persons compensated for their inefficiency by repressing subjective disturbance and checking items indicating no unusual upset. This is related to Strother's (15) finding that *Ss* with high (unfavorable) scores on a psychoneurotic inventory use more ego protective types of defense mechanisms.

These results corroborate Hines's finding (8) of no clear relationship between *E*'s rating of the *S*'s overt adjustment to the frustration (based in part on the remarks he made spontaneously during the experiment and what he wrote about it afterwards when asked to describe the effects of the experiment on him) and objective scores which indicated efficiency in the task experimentally disturbed. These results may also be related to Strother's (15) inability to find greater relationship between efficiency and the personality adjustment questionnaire, and Hines's (8) failure to find a relationship between efficiency and questionnaire case history items.

It is definite that there is no relationship between an individual's efficiency in a frustrating situation and what he says about his attitude toward the situation.

Inspection of Table 4 shows that the per-

centage of subjects indicating enjoyment or no unusual disturbance was much greater than that admitting disturbance. Inspection of individual papers indicates that 31 per cent of the *Ss* checked at least two items each in both categories, probably indicating both disturbance and attempts at rationalizing and adjusting to it.

19.77) and 18.18. These *SDs* were 5.10 (11.96, 13.35) and 8.61, respectively. From the group she took 18 *Ss* who showed the greatest increase in error under experimental frustration and 16 *Ss* who showed the least increase. These *Ss* were given the Rorschach ink blot test, and the responses of the two groups were compared. Rorschach differences were found between the stable and labile groups in respect to error increases under experimental frustration. These differences were in the expected direction, but only one *mean* difference between Rorschach scoring categories was statistically significant. The following scoring categories separated the two groups reliably in terms of *variability* of response at the 1 per cent level: time per response, time of first response to achromatic

TABLE 4

QUESTIONNAIRE RESPONSES OF A GROUP WITH LARGE ERROR INCREASES AND A GROUP WITH NEGLIGIBLE ERROR INCREASES

ITEM No.	ITEM	PERCENTAGE OF GROUP WITH LARGE ERROR INCREASE $N = 112$	PERCENTAGE OF GROUP WITH NEGLIGIBLE ERROR INCREASE $N = 24$
	Admission of Disturbance		
5.	I wish tests like this didn't bother me as much as they do.	26	38
10.	I had a hard time to keep from moving around in my chair.	29	46
13.	I wish I were not so nervous on a test like this.	20	25
16.	I have always gotten excited in situations like this.	12	29
31.	I feel like cussing and letting off steam—I think this would have helped.	16	17
51.	I wondered how many were doing worse than I was.	45	58
56.	Tests like this bother me but I think I'll get over it in time.	17	25
62.	I kept thinking of how rotten I was doing and this made me make more errors.	05	13
3.	That test got my goat. I was ready to hit myself on the head.	14	04
4.	I try not to let a test like this confuse me, but I almost went to pieces once or twice during this test.	25	33
6.	I am just too nervous on a test like this to do any good.	13	13
61.	I wanted to quit because this made me jumpy, but I kept on.	15	21
63.	I got pretty badly mixed up during this test.	12	29
	Statement of Enjoyment or No Unusual Disturbance		
15.	I just can't get too bothered during a test like this.	25	17
21.	I knew I was doing as well as the next fellow.	53	50
24.	I am sure I was no different than most of the fellows in the room.	75	71
29.	I like a test of this kind; it keeps me on my toes.	51	42
38.	It was hard, but I like hard tests.	48	29
44.	I thought while I was doing this test, "I'm doing my best, and that is all a man can do."	65	67
54.	I never let a test like this bother me; I just do what I can do.	70	58
55.	I feel like I've had a "work out," but I didn't lose my head, even though I didn't pass.	66	67

Rorschach Responses and Efficiency Scores

The entire experiment was repeated identically by Allee (1), using an unselected group of 98 post-war college students. The results were highly similar in all respects. The means of problems attempted under the three conditions were 55.79 (Sec. 1, 73.30; Sec. 2, 62.32) and 77.70, respectively. The *SDs* were 12.80 (17.83, 19.88) and 16.61. The errors for the three conditions were 7.44 (21.62,

and color cards, occurrence of *F—*, *M*, and *C*, and *F*-ratio; at the 5 per cent level: percentage *FC* to *F*, and number of *FK*.

It appears, in conclusion, that whereas there is a positive reliable relationship between behavior under experimental frustration and behavior shown through some Rorschach scoring categories, the discriminative value and underlying rationale of the two techniques are sufficiently independent to warrant experimental use of both in clinical situations.

DISCUSSION AND CONCLUSIONS

The degree of inefficiency and the dispersion of error scores are greater in this study than in either the studies of Strother (15) or Hines (8). The technique of having the experimenter interrupt *S* every half-minute and emphasize his failure to meet the requirements of the task was effective. It suggests that Hines's mechanical means (8) of calling attention to "inadequate performance" was the reason for fewer cases of disturbance of efficiency. The group pencil and paper approach does not decrease the effectiveness of the frustration.

This technique is an effective and economical one to produce experimental frustration and to isolate individuals showing different kinds of response under the conditions. It opens the way to a study of the development of frustration proneness and tolerance. Moreover, it can be used as a clinical tool, and the relationship of this kind of behavior to other personality traits and stimulating situations may be explored.

These results lead to the following conclusions: (1) Individuals differ in their reactions to experimentally frustrating conditions; (2) frustration can be produced by a group pencil and paper technique; (3) two patterns of behavior have been discovered that differ widely: (*a*) a relatively steady performance with no increase in errors and, (*b*) an increase in speed or output but with marked increase in error; (4) there is no significant positive relationship between inefficiency during frustration and disturbance as indicated in a verbal expression of feelings and attitudes; (5) some slight relationship exists between variability in certain Rorschach responses and behavior under frustration.

SUMMARY

An attempt has been made to measure the effect of artificially induced frustration on performance and to relate these to the *S*'s admission of emotional disturbance. A "Reporting Test," which required rapid discrimination and counting of figures in a series of circles, was used. After an initial period of performance at a rate set by each *S* and without interference by the examiner, the task was continued but with interruption at the end of each half-minute, which indicated to *S* that he was failing to meet a standard of performance. This served to produce the experimentally frustrating condition. After two eight-minute periods of performance under frustration, a final eight-minute period of work without pressure was required. All the *S*s filled out a questionnaire, checking items which described their feelings and attitudes during and after the experiment. One hundred ninety-six Naval radar operator trainees served as *S*s.

Under speed stress or frustrating conditions the *S*s, as a group, attempted more problems, made more errors, and showed greater variability of performance. In general, efficiency was reduced. However, not all individuals reacted in the same way to experimental frustration: the majority speeded up performance at the expense of accuracy, but a smaller group showed stable performance without increase of speed or errors.

In terms of this experiment, it appears that there may be two gross reactions to speed-stress of experimental frustration: (1) an increase in speed and errors with relative inefficiency, and (2) stable performance and efficiency with little increase in speed and errors. The results suggest that individuals showing the latter type of reaction constitute a small group and might be recommended for critical tasks where errors may be expensive in personnel and equipment. Individuals whose reactions are of the former type might be used for tasks emphasizing speed but not requiring great accuracy.

There was no positive reliable relationship between attitudes and feelings expressed on a questionnaire, and efficiency; there is a relationship between efficiency and variability in some Rorschach responses.

This technique has potential value as a

clinical tool and as an experimental method for the investigation of the generality and development of frustration tolerance and proneness.

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AN EXPLORATORY INVESTIGATION OF AUTISTIC PERCEPTION

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In spite of extensive investigation, the question of whether or not motivation can distort perception still lacks an experimental answer (cf. 12). The "rich man–poor man" design (2, 3) is fundamentally unsound; and an unwarranted extension of the meaning of "perception" has characterized other studies (1, 7, 8, 9, 10, cf. 14, 15). More specifically, the experimental approach to "autistic perception" (11) has been characterized by paucity of subjects, triviality of incentive, and lack of precautions against falsification of subjective report (4, 5, 13, 16). In view of the recognized theoretical importance of the problem (2, 6, 11, 17), the present investigation undertook to explore autistic perception on a fairly rigorous basis.

Procedure

The investigation reported here was publicized as a psychology experiment which was to take the form of a contest in span of visual apprehension.[2] A first prize of ten dollars was offered, and a runner-up prize of five dollars. Forty-six male undergraduates were thus recruited. They were divided at random into two groups of 23, and a separate "contest" was staged for each group.

When a member of either group reported for participation in his contest, he found himself in a lecture room containing ten rows of desk-chairs, on a floor which sloped upward toward the rear of the room. The *S* was handed an answer sheet which indicated the seat he was to occupy. As finally disposed, the *S*s in either group occupied 23 alternate seats in the central portion of the room.

At the front of the room hung a large motion-picture screen with a black fixation point at its center. At the rear of the room stood a projecting tachistoscope, adjusted to provide an exposure time of one-tenth of a second. Room illumination was somewhat below normal, allowing adequate projection as well as the use of answer sheets.

To each group the same *E* read a standard set of instructions. It was explained that this was an experiment in visual perception of number, but that prizes were being offered to insure motivation. Clusters of white dots were to be flashed upon the motion-picture screen, and each *S* was to attempt to decide how many dots there were in each case. To illustrate the procedure, ten practice clusters were presented.

When the *S*s had been familiarized with the experimental procedure, the scoring system was explained to each group. In neither group were answers to be forced. In both groups, wrong answers were to be punished heavily and equally: there was to be a ten per cent deduction from the *S*'s score for each of his incorrect responses. The sole difference between the experimental treatments of the two groups occurred in the instructions as to scoring of correct responses: an *S* in the *control group* was to receive one point credit for each *cluster* correctly reported; an *S* in the *experimental group* was to receive one point per *dot* in each cluster correctly reported.

The experiment proceeded for each group. Fifty clusters were flashed on the screen, at the rate of 1 per 20 seconds. Each cluster consisted of from five to nine 1½-inch white dots, irregularly spaced within 11 inches of the black fixation point. The *S*s filled out their answer sheets and turned them in.

[1] The experiment reported in this paper was carried out by the two junior authors, with the direction and assistance of the senior author, in fulfillment of Senior Thesis requirements at Princeton University (1947–1948). The authors wish to acknowledge the financial support very kindly provided by the Department of Psychology of that University. The authors wish also to thank for their assistance in statistical considerations Dr. J. K. Adams, Dr. W. E. Kappauf, and Dr. H. O. Gulliksen, all of Princeton University; and Dr. C. C. Peters and Dr. A. K. Kurtz, both of The Pennsylvania State College. The statistical treatments employed are, however, the responsibility of the authors alone.

[2] For detailed account of both procedure and results order Document 3034 from American Documentation Institute, 1719 N Street, N.W., Washington 6, D.C., remitting $1.00 for microfilm (images 1 in. high on standard 35 mm. motion picture film) or $2.70 for photocopies (6 x 8 in.) readable without optical aid.

The sheets were immediately scored in the appropriate manner, and the prizes were awarded before the *Ss* left the room.

RESULTS AND DISCUSSION

The rationale behind these procedures was fairly simple. By not forcing answers and by imposing a heavy penalty upon incorrect answers, an effort was made to insure the honest report of only such compelling judgments as could legitimately be called perceptions. Within this framework, both groups were motivated toward strict accuracy of report, but the *Ss* of the experimental group were provided with a conflicting motivation to see as many dots as possible. The question was whether or not the latter motivation would find expression in autistic, exaggerated reports.

To answer this question there were tabulated, for each cluster separately, the following figures: (1) frequency of response (*correct or incorrect*) for the experimental group, minus frequency of response for the control group; (2) frequency of *correct* response for the experimental group, minus frequency of correct response for the control group; (3) frequency of *overestimation* for the experimental group, minus frequency of overestimation for the control group; and (4) frequency of *underestimation* for the experimental group, minus frequency of underestimation for the control group. Working, then, within each of these categories of tabulation, the average frequency difference was found for clusters 1–5, 1–10, 1–15, 1–20, 1–30, 1–40, and 1–50, respectively. Finally, each average frequency difference was tested to determine whether or not it was significantly different from zero.

The upshot of this procedure can best be typified by saying that the summation at the fifteenth item showed the following trends. The experimental group was more responsive (at the .02 level of confidence); however, it also tended (at insignificant levels) to give *fewer* correct responses and *fewer* underestimations. The excess responses of the experimental group were evidently devoted to exaggeration, for that group generated *more* overestimations than the control group, at a level of confidence of .002. The figures at the tenth item and at the twentieth were consistent with the picture provided at the fifteenth. At the tenth item, the experimental group showed more frequent overestimation at the .02 level; at the twentieth item, at the .05 level. Beyond the twentieth item the two groups converged in performance with respect to overestimation and underestimation; the experimental group, however, showed significantly greater responsiveness and, eventually, significantly more frequent correct responses.

The results, then, seem to be quite consistent with the assumption that autistic perception was induced in the experimental group during the early phases of the experiment. It should be carefully pointed out, however, that the statistics cited apply only to the two groups used in this particular experiment. That is, the universe sampled was that of the responses of these groups, and not that of the responses of "people in general." The more general tests were of course applied. Although the trends naturally remained, it proved impossible to demonstrate their significance on the broader basis. Thus, the present results should be viewed with some degree of skepticism until they can be independently verified in more general terms.

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PROBLEM SOLVING BY SMALL GROUPS USING VARIOUS COMMUNICATION NETS[1]

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A TREND can be discerned in the history of the experimental studies of small groups. Early studies determined that the presence of other people has an effect upon individual performance. Competitive and noncompetitive situations were compared, but social interaction and cooperation was slight (2). More recently the small group has been studied as a unit. The group is given a problem to solve; the discussions of the group are recorded (1), or the efficiency of the group is compared to that of a single individual (6). From Lewin and his followers have come studies of interaction in differently organized groups and of the effectiveness of these groups in coping with various situations. The trend seems to be toward more detailed analysis of the group structure and more interest in the processes of interaction among the members of the group.

Still more precise specification of the group structure and of the kind of interaction has been achieved by Bavelas and his associates. These workers control the channels of communication among the members. Components of the solution are given to each person, but all members of the group must cooperate to reach the correct solution.

In Leavitt's experiment (4), for example, five *S*s were seated around a table, but separated from one another by vertical partitions. Written notes could be passed through slots in the partitions. By varying the slots that were open, the channels among the five could be manipulated into any desired pattern. Four patterns were tested: (1) In the *circle* each person could pass notes to the person to his right or to his left; (2) the *chain* was identical to the circle except that one more slot was closed, so the *S*s on either side of this closed slot found themselves at the two ends of a chain; (3) the Y was a four-member chain, and the fifth *S* could exchange notes only with one of the inner members of this chain; and (4) the *wheel* (or X) put one *S* at the center of the net in such a way that he could exchange messages with all the other members, and the other four could not exchange information without passing it through the central member.

Each *S* was given five different symbols out of a possible set of six. The task was for the entire group to discover the one symbol held in common by all five members. Records were kept of speed, errors, and number of messages. At the end of the experimental session the *S*s were given a questionnaire before they talked to each other.

The circle was the most active, erratic, unorganized, and leaderless, but was most satisfying to its members. The wheel was least erratic, required relatively few messages to solve the task, was organized with a definite leader, but was less satisfying to most of its members. The chain and the Y were more like the wheel than like the circle. The member in the central position became the leader, and was more satisfied with his job than were the occupants of peripheral positions. The different communication nets did not differ significantly in the average time it took to solve the problem.

The present experiment is similar in many respects to Leavitt's, although the control of the situation is carried still further. Each *S* is required to reach a solution of the problem. The necessary information is divided equally among the *S*s. Speech, instead of written messages, is the method of communication between group members. However, the content of the messages an *S* can send is restricted and suitable for quantification. The intelligibility of the speech was controlled during a test by controlling the relative intensities of the speech and the noise.

[1] This research was carried out under Contract N5ori-76 between Harvard University and the Office of Naval Research, U.S. Navy (Project NR142-201, Report PNR-93). Reproduction for any purpose of the U.S. Government is permitted.

These controls permit determination of the dependent variable, group performance, as a function of the independent variables: (1) group organization, (2) intelligibility of the message, and (3) type of problem.

APPARATUS AND PROCEDURE

Nets

The communication nets were set up as follows. Three *S*s were located in three adjoining rooms. Each *S* had a microphone, amplifier, and earphones. Listening was binaural except when a listener was connected to two talkers; then the listener heard one talker in one earphone and one in the other. The five nets tested are shown schematically in Figure 1. The direction of the arrows indicates the channel from talker to listener. A two-headed arrow signifies two-way communication. The nets will be referred to by number, 1 to 5. The subjects will be referred to by letter, *A*, *B*, or *C*, according to the positions indicated in Figure 1. All the channels were free of distortion, and passed frequencies from 200 to 7000 cps.

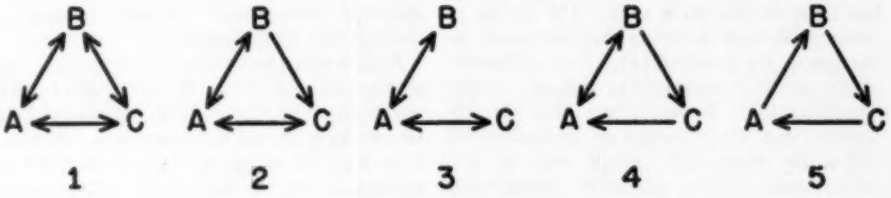

FIG. 1. THE FIVE NETS

The arrows indicate the direction of communication from talker to listener.

Noise

Random noise is a shishing sound that has all frequencies of vibration present at equal intensities. Because of the similarity to a white light that has all wave lengths present, such a sound is often called white noise. White noise is a very effective masking sound. This noise was introduced in equal amounts into all the channels. Some noise was always present during the tests in order to mask any airborne sounds that passed directly through the walls.

The intelligibility of speech in the presence of noise is a function of the relative intensities of the speech and the noise, and is independent of their absolute levels over a wide range (3). In order to control the amount of external stress introduced, the speech-to-noise ratio was set to a given value by adjusting the intensity of the noise. Each *S* was given a voltmeter that indicated the intensity of the electrical signal generated by his voice. This meter was used to monitor the speech signal at a constant level (approximately 80 db re 0.0002 dyne/cm^2 at the listener's earphones). Use of the meter counteracted the natural tendency of talkers to raise their voices as more noise is introduced. In this way the likelihood of errors could be controlled by the experimenter. Preliminary tests showed that 85 per cent of the monosyllabic words (from a memorized list of 256) could be received correctly at a speech-to-noise ratio of +6 db, 66 per cent at −2 db, and 24 per cent at −10 db.

Problems

The problems were of three kinds. Problems used in Experiment I called for a comparatively stereotyped and unimaginative exchange of isolated words. The problems in Experiment II provided more opportunity for initiative in the construction of sentences. Experiment III was based on a kind of anagram problem. These problems are described in more detail in the discussions of the three experiments.

Subjects

The three *S*s in the word tests were undergraduates in Harvard College. They had been *S*s for 15 hours of articulation testing during the preceding month. These *S*s had become thoroughly familiar with a master list of 256 common monosyllables. Two of these three *S*s continued through Experiment II; the third was replaced by a graduate student who quickly mastered the procedure under the tutelage of the other two. After the first few trials the new member performed as well as the others. Two groups of three naïve *S*s, all graduate students, were recruited for the anagram tests of Experiment III.

EXPERIMENT I: TESTS WITH WORD PROBLEMS

Method

In order to explain the construction of the word problems, the following terms are defined. (1) The *master list* is a set of 256 monosyllables from which all test words were drawn. These 256 words, arranged according to vowel sounds, were placed in easy view of each *S* throughout the word tests. (2) The *test list* is a list of 25 words selected at random from the master list. (3) The *subject's list*

is a list of pairs of words selected at random from the test list. The first step in the construction of a word problem is to assemble a test list of 25 words. Then the pairs of consecutive words from the test list are collected in three sets to form three sub-lists, one for each *S*. Part of a sample test list and the three *S*s' lists derived randomly from it are shown in Table 1. The word pairs are given to the *S*s in their correct order as they appear on the test list.

TABLE 1

SAMPLE OF TEST MATERIAL

The three subjects received their lists in this form, and attempted to reconstruct the test list cooperatively.

TEST LIST	SUBJECT *A*'s LIST	SUBJECT *B*'s LIST	SUBJECT *C*'s LIST
south	though	south	south
though	off	though	
off			plod
quiz	off	grade	sniff
grade	quiz	act	
act			pounce
dwarf	quiz	sin	rash
plod	grade	whiff	
sniff			rash
pounce	act	range	gun
rash	dwarf	sledge	
gun			gun
coast	dwarf	wire	coast
pig	plod	nine	
sin			coast
whiff	sniff	raid	pig
pent	pounce	jug	
cook			whiff
range	pig	shaft	pent
sledge	sin	fake	
comes			sledge
fort	pent	fake	comes
wire	cook	by	
nine			etc.
etc.	etc.	etc.	

The problem is to reconstruct the test list in its entirety. Since each *S* has only a part of the information needed for the reconstruction, he is forced to communicate over the net provided. In the example of Table 1, *C* begins by saying "You will write 'south.'" (The carrier phrase "you will write . . ." makes it possible to warn the listeners and to monitor the speech level before the test word occurs.) The word "south" travels through the net until it reaches *B*, who then introduces "though." When "though" reaches *A* he introduces "off," "quiz," "grade." The details of the process are evolved by the *S*s themselves during the preliminary testing, but in this general manner the test list can be patched together. Every *S* must get the entire test list of 25 words before the problem is completed.

Before each test the *S*s were carefully informed about the pattern of channels. They knew that the speech-to-noise ratio was always the same in all channels. Discussion of the roles of members located at different points in the net was discouraged. No suggestions were made about the strategy that the group might employ in solving the problem. The *S*s were not instructed to solve the problem in the shortest possible time; they were told to proceed at a reasonable rate consistent with the accurate reconstruction of the test list by all three participants. The *S*s were permitted to speak only the carrier phrase followed by any number of the words on the master list. A talker was not allowed to indicate to whom he was speaking. These restrictions on the group's vocabulary maintained the intelligibility of their speech at a known, constant level. The order for testing the nets and the positions of the *S*s in the nets was determined by a pre-arranged plan.

Four three-hour experimental sessions were required before the group performance stabilized enough to permit satisfactory comparisons of different trials. The times required for completion of the problems decreased steadily during the preliminary period as the *S*s became familiar with the problem and the best procedure on the various nets. The rate of learning was too rapid for accurate assessment of the influence of the experimental variables, although the same general relations obtained as in the later trials that are reported in detail.

The *S*s found the problem an interesting one. Post mortems were a common occurrence after a trial was over, especially when a persistent error had hampered progress. During the trial several spontaneous expressions of agitation were noted. Frustration and irritation were evident on net 5 at the lower signal-to-noise ratios, where correction of errors was difficult and progress was slow. Once, in net 1, an *S* complained that the other two were monopolizing the conversation. In net 3 both subjects *B* and *C* occasionally talked at the same time to a harried subject *A*. Frequent reminders were necessary to prevent the *S*s from raising their voices, thus improving the signal-to-noise ratio, when attempting to correct errors.

By the end of the preliminary period of rapid improvement the group was a smoothly functioning unit within the limits imposed by the three signal-to-noise ratios and the five nets. The *S*s evolved the practice of reporting both members of the word pairs in the same message, which tended to keep all *S*s "up to date" and the words in their proper context.

Results

The results reported in Table 2 and Figure 2 were obtained in two three-hour experimental sessions. Three kinds of measures are reported. (1) The *time* required to complete the task was measured from the first "You will write . . .," spoken by the *S* who had the first word, to the point when each *S* had compiled his 25-word list. (2) The *accuracy*

of the completed list was scored by comparing the *S*'s written record with the test list. (3) The *number of words* spoken by each *S* was recorded by him as he said them. The totals for all three gave the number of words the group had spoken in order to solve the problem.

A reconstruction of the course of solution would have been possible if the *S*s had written down all the words they said. It proved impossible to write out all the words spoken without destroying the tempo and spontaneity of the group interaction. Hence the *S* was asked to record only the *number* of words he said, rather than the actual words themselves.

The minimum curve plotted in Figure 2A represents the minimum total number of words that the group could say for each *S* to conclude with a reconstruction of the 25-word test list. The calculation assumes no articulation errors by any *S*, and no deviations from the maximally efficient procedure for a particular system. The minimum times of Figure 2B are obtained by dividing the minimum word scores by 13, the average number of words spoken per minute. The similarity between Figure 2A and Figure 2B indicates that the *S*s tended to talk at an approximately constant number of words per minute under all conditions.

It will be noted that the results for a signal-to-noise ratio of +6 are strikingly similar to the minimum curve except for a displacement upwards. Few errors in communication occur at this favorable signal-to-noise ratio. Consequently the difference between minimum and empirical curves indicates a consistent use of surplus words. Unnecessary repetitions and the habit of saying both the words of a pair provided the surplus. The *S*s were not instructed to be economical with words or time.

Figure 2 shows that raising the noise level, with the consequent introduction of errors, accentuates the differences between the systems. The comparative inefficiency of net 5, the one-way closed chain, becomes clear when

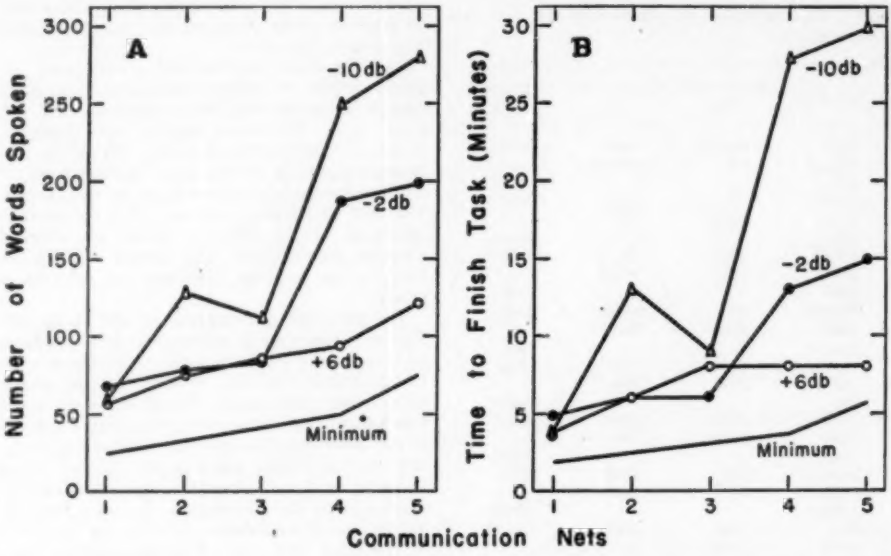

FIG. 2. PERFORMANCE OF THE GROUP ON THE WORD PROBLEMS (EXPERIMENT I) AS A FUNCTION OF THE COMMUNICATION NET

Nets are numbered as in Fig. 1. Performance is measured in terms of (A) total words spoken and (B) time required to finish the task. Parameter is signal-to-noise ratio.

the noise is introduced. In this net one person's failure to hear correctly a particular word stalls the group's progress; locating the culprit is difficult and correcting him harder still. The same message, containing an error stubbornly introduced by one member and as stubbornly corrected by another may go round and round net 5 before the mistake is finally corrected. Short periods of silence, extremely unusual in other systems, occurred often enough to decrease appreciably the average number of words per minute. The deficiencies of net 5 are present in a less extreme form in net 4, where only one of the *S*s was in the semi-isolated position.

The resistance of net 1 to the deleterious effects of noise is readily explained. Each *S* starts the problem with more than a third of the total information required for solution. He is provided with the words he must contribute and also the words that precede them. Thus in net 1, one of the two listeners to every word must have the word (if it is correct) written down in front of him. Each listener makes what is essentially a two-alternative decision. He decides whether the word he hears is the one on his paper, in which case he introduces the second word of his pair, or whether it is some other word. The articulation score for a two-alternative vocabulary is quite high (5). The listener in this situation is likely to make the correct decision. Incorrect decisions are immediately detected when neither, or both, of the *S*s introduces a next word.

The number of words spoken by a member of the group is determined by his position in the net. The proportional participation of each member is approximately the same as the proportions that can be calculated by considering the most efficient possible solution.

Casual observations indicated that *S*s in different positions in the nets play different roles. The man in position *A* of net 3 is forced by his central position to become a coordinator. He usually warns the other two members at the beginning of a trial, and takes charge of any procedural matters that arise during the trial. When asked at the conclusion of the experiment which position in the network they preferred, all *S*s chose this central position. Isolated positions, of which the clearest example is *C* in net 4, were unpopular with the *S*s, who reported that in these positions they felt left out and unsure of themselves.

A stereotyped procedure, in which the *S* waits passively until he hears the first word of a pair on his list before introducing the second word of his pair, is quite appropriate for the solution of these word problems. The correspondence of the empirical results to the predictions of group performance—predictions based upon the assumption of such machine-like behavior—shows that the word problems did not provide a real challenge for the group's abilities.

TABLE 2

GROUP SCORES FOR WORD PROBLEMS

NET	THEORETICAL MINIMUM NO. OF WORDS	THEORETICAL MINIMUM TIME (in minutes)	PER CENT WORDS CORRECT			TOTAL WORDS REQUIRED			TIME TO FINISH TASK (in minutes)		
			+6db	−2db	−10db	+6db	−2db	−10db	+6db	−2db	−10db
1	25	1.9	97.5	94.7	80.0	56	68	58	4	5	4
2	33	2.5	97.5	94.7	86.7	75	77	128	6	6	13
3	42	3.2	98.7	85.4	96.0	86	85	112	8	6	9
4	50	3.8	96.0	94.7	84.0	94	186	248	8	13	28
5	75	5.8	96.0	90.7	82.7	122	198	279	8	15	30

EXPERIMENT II: TESTS WITH SENTENCE PROBLEMS

Method

The sentence problems relied upon verbal context, rather than the repetition of word pairs, to link the contributions of the three members. These problems were less rigidly structured and provided more scope for initiative. The test materials consisted of sentences of 25 words, as simple in thought and expression as possible. For example, "The picture we saw was painted by an old woman who had been taught how to mix the colors by one of the native artists." All sentences used were of approxi-

mately equal difficulty and were designed (1) to minimize punctuation, common phrases, and clichés, and (2) to express thoughts unrelated to any particular knowledge possessed by the *Ss*. The words in the sentences were commonplace; no proper names and no words longer than three syllables were used.

The words in the sentences were divided into three parts with the aid of a table of random numbers. At the beginning of a trial each *S* received his portion of the words. The words were listed vertically in the order in which they occurred in the test sentence. As before, the task of the group was to reconstruct the original sequence of words in their correct order; each member had to conclude with the complete sentence.

The same five networks and three speech-to-noise ratios were investigated as for the word problems, but more attention was paid to nets 1, 3, and 5, and more data are available for them. The *Ss* received the same instructions; they could say as many words as they wished, preceded always by the warning "You will write . . ." The messages actually sent were somewhat longer than they had been for the word problems, because the intelligibility could be raised by placing the words in the context of the sentence.

Sentence building provided a problem that held the *S*'s interest. Each sentence posed a novel problem. Discussions, in which the correct sentence was disclosed and jocular recriminations passed among the *Ss*, were unavoidable after trials that were difficult or delayed by errors.

Records were kept of (1) the time required to complete the problem, and (2) the number of words spoken by each *S*. Accuracy scores were not useful. An *S* could tell when he had the correct sentence by observing that he had 25 words strung together in a reasonable sequence that included, in the proper order, the words he had been given at the outset. The *Ss* seldom missed more than one or two words of the test sentence.

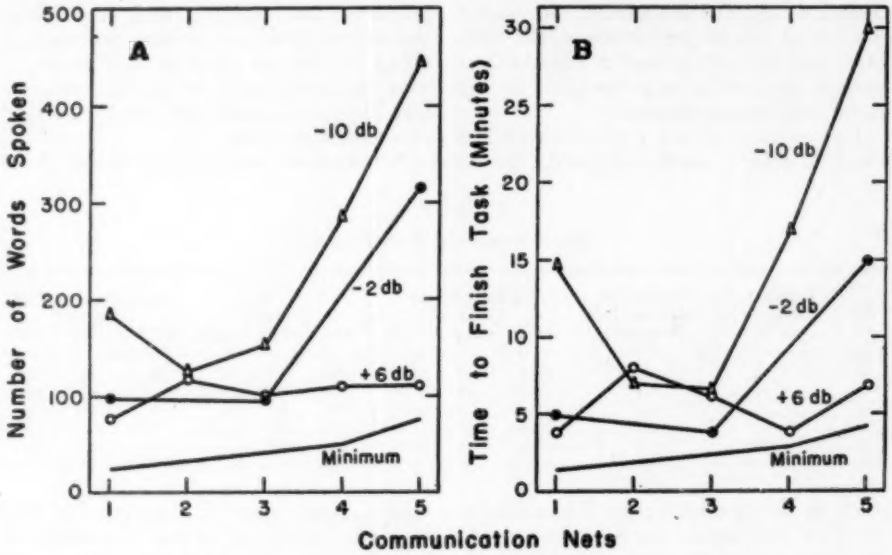

FIG. 3. PERFORMANCE OF THE GROUP ON THE SENTENCE PROBLEMS (EXPERIMENT II) AS A FUNCTION OF THE COMMUNICATION NET

Nets are numbered as in Fig. 1. Performance is measured in terms of (A) total words spoken and (B) time required to finish the task. Parameter is signal-to-noise ratio.

Results

Time and total word scores are given in Table 3 and plotted in Figure 3. The results indicate that net 3 is superior to net 1. This superiority is most marked for the unfavorable speech-to-noise ratios, which shows once more that the stress of noise exaggerates differences among the nets.

In net 1 everyone always talks to everyone else. In net 3 a man in the middle is the go-between for the other two. Why is net 1 superior for word problems but inferior for sentence problems? Apparently the recon-

struction of sentences requires more integration of group activity; the central man can coordinate and place in the proper context the words that the *S*s contribute. He is in a position to detect and correct mistaken words and incorrect hypotheses. Conversely, the situation can become chaotic in net 1, for no one organizes the individual contributions. These considerations could mean that the proportional participation of the central man in system three is higher for the sentence problem, where he plays a more significant role, than for the word problem. The individuals' data show, however, that the proportional contributions for the central man are approximately equal for both kinds of problem.

EXPERIMENT III: TEST WITH ANAGRAM PROBLEM

Method

The anagram problem was taken from a study by Watson (7). The group was given a word and instructed to form as many anagrams as possible. The test words were *abolished, courtesan, educators, neurotics, secondary,* and *universal.* All had nine letters, four vowels, and the consonant *s.* The words formed had to be at least three letters long; singular and plural did not count as separate words; proper nouns and slang were not permitted. The group's goal was to get the largest possible number of words on all three papers; a word could count three times toward the group total if it appeared on all three papers. Communication was not essential for carrying out the task. The extent to which *S*s communicated the words they made up, or heard from other *S*s, was left completely to their own discretion.

The instructions in the use of the equipment were the same as before. Only three nets were tried with anagram problems, nets 1, 3, and 5. Two speech-to-noise ratios, −10 and +6 db, were tested. The group was allowed ten minutes to work on each word. Three practice tests, one with each net, accustomed the *S*s to monitoring their voices and to constructing anagrams.

Results

The results for the last six tests are summarized in Table 4. These figures are the average values obtained for two groups of three subjects.

It is clear from Table 4 that the differences among the nets were small, and that the introduction of noise did not emphasize the differences among the nets. For this kind of problem, therefore, the pattern of channels among the members of the group has little effect upon the efficiency of the group's performance. The unfavorable speech-to-noise ratio interfered with the group's performance in about the same degree for all nets.

The *S*s marked on their score sheets all the words that they spoke aloud. The total number of words (average for the two groups) that were spoken during the 10-minute tests are shown in Table 4. Fewer words were spoken over the multi-connected net 1 than over the minimally-connected net 5. Intense noise decreased the amount of talking.

In the word and sentence problems of Experiment I and Experiment II, the efficiency of the group depended upon the pattern of channels available for communication among

TABLE 3

GROUP SCORES FOR SENTENCE PROBLEMS

NET	THEORETICAL MINIMUM NO. OF WORDS	THEORETICAL MINIMUM TIME (in minutes)	TOTAL WORDS REQUIRED +6db	−2db	−10db	TIME TO FINISH TASK (in minutes) +6db	−2db	−10db
1	25	1.4	76	99	186*	4	5	15*
2	33	1.8	117		121	8		7
3	42	2.3	101*	96	154*	6*	4	7*
4	50	2.8	112		287	4		17
5	75	4.2	112	315	446*	7	15	30*

* Averages for two problems.

TABLE 4

AVERAGE GROUP SCORES FOR ANAGRAM PROBLEMS

NET	TOTAL WORDS CONSTRUCTED +6db	−10db	WORDS SPOKEN +6db	−10db
1	165.5	149.5	61.5	64.0
3	167.0	142.5	93.0	81.0
5	176.0	141.0	135.5	99.0

In the word and sentence problems of Experiment I and Experiment II, the efficiency of the group depended upon the pattern of channels available for communication among

the group members. In the anagram problem, the type of net apparently made no systematic difference in the efficiency of the group. This difference can be explained on the basis of two principal ways in which the word and sentence problems differ from the anagram problems.

(1) The information necessary to solve the anagram problem was nonconsecutive and was not divided among the group members. All group members began the problem with identical information which was adequate for the assigned task. In contrast to the word and sentence problems, where the *S* had to wait for the contribution of another group member before he could proceed, the *S* in the anagram problem could work at the task in his own preferred order, at his own optimum speed. Consequently errors and delays in the transmission of words did not appreciably hinder an individual's productivity. The correction of errors, a procedure much easier in some nets than in others, was unnecessary in the anagram problem for keeping the group at work. Errors in the transmission of words frequently stalled progress on the word and sentence problems, however, because an *S* often could not introduce the next word until he had correctly heard the preceding word.

(2) The task assigned an *S* in the word and sentence problems was primarily to communicate to the other group members the information that had been presented to him at the beginning of the problem. Consequently, the performance of the group depended in large measure upon the ease of communication. In the anagram problem, however, the *S* himself had to make up the words he sent out. Table 4 shows that *S*s made up about five anagrams per minute, and that not all of these were sent over the communication net. Even the most inefficient system was adequate for this slow rate of output; it is not surprising that no significant differences were observed for the various nets.

DISCUSSION

A few generalizations can be drawn from these experiments. The relative efficiency of a communication net depends upon the kind of problem the group is trying to solve. The nature of the group's task is the most important variable that remains to be explored. Exposing a group to unfavorable conditions can exaggerate differences in the relative efficiency of different nets. However, if the problem does not require successful communication, the efficiency of the group may be little affected by the net pattern or by noise.

The experimental situation investigated here is considerably more restricted than most of the group situations studied in the laboratory or in the social interaction of day-to-day life. To what extent does this artificiality limit the generality of the results? Some of the limitations of the method can be overcome by modification or enlargement of the equipment. Additional persons, and additional group organizations, can be introduced by adding microphone, loudspeaker, amplifier, and separate room for each person. The size of the group is an important variable that we have not considered here. A switching system can easily be installed that would allow the *S*s themselves to determine the group organization and the recipients of particular messages. Differential noise conditions in the channels can be used to force some of the group members into relatively insignificant roles and to give relative prominence to others. Wire recording can be used for a more thorough analysis of each individual's contribution, the origin of errors, and the progress of problem solution.

Less easily overcome are the limitations imposed by the range of problems appropriate for this experimental situation. Quantification of results is greatly facilitated by restriction of vocabulary and standardized procedure. Since the problem material must all be expressed verbally, the results are perhaps unduly influenced by factors such as verbal context and intelligibility, with a reduction in general applicability to the less restricted life situation.

One thing that is needed is a classification of group problems. What are the significant dimensions of a problem? One obvious dimension is the degree to which the problem is structured. A continuum, based on the degree of structure of the problem, would run from problems so highly restricted that an individual group member could be replaced by a fairly simple robot to the problem faced

by a town council deciding whether to raise taxes. The problems devised for these experiments are far toward the structured end of the continuum. At the other end of the scale are problems investigated by the Lewin group, where factors such as the role played by various group members and their personalities, attitudes, and needs were involved explicitly. Many of these factors could be introduced systematically into the intercommunication system employed in this work by a relaxation of curbs on the members' activities, and the setting of more broadly defined tasks.

Other dimensions include such possibilities as the uniqueness of the solution, the initial distribution of information, the number of decisions that someone must make, the rate at which a participant can narrow the range of possible solutions, the position along a competition–cooperation continuum, etc. With a reasonable classification of problems we can begin to relate the problem, the net, and the stress in a systematic way.

It is possible that stress may act as a microscope to magnify differences that otherwise are difficult to measure. How general is the stressful effect of noise? Do other kinds of stress have comparable effects? To what extent can the activity of one member be regarded as a source of stress to another member of the net?

By their careful controls of the conditions under which a group must work, Bavelas and his co-workers have extended the precision of research with small groups. If this precision does not cost too much in validity, we may have a powerful experimental method.

SUMMARY

The performance of a three-man group was studied for five different communication nets, three signal-to-noise ratios, and three kinds of tasks. The first type of problem was a simple reassembling of a list of standard words. The second type consisted in the construction of a sentence, the words of which had been distributed among the group members. The third type required the group to form anagrams.

For the first type of problem, comparative group efficiency, measured in terms of time or number of words required to complete the task, could be predicted from the net structure. A closed chain in which only one-way communication was possible between any two persons was by far the least efficient; an open chain, which allowed two-way communication between any two adjacent individuals, was intermediate; a closed chain where all members talked and listened to all other members was most efficient. The second type of problem was less rigidly structured and placed a higher premium on the coordination of the group activity. The results were generally similar to those for the first type of problem, except that the open chain, which had a man in a central coordinating position, replaced the two-way closed chain as most efficient. The anagram problem did not require communication; under these conditions there was no large difference among the nets.

Lowering the signal-to-noise ratio introduced errors and increased the time and number of words required to complete the task. For the first two types of problems the stress of noise accentuated the differences between the systems and emphasized the inefficiency of the one-way closed chain. The third problem, where communication was a luxury, was solved equally well over all nets; noise did not accentuate differences.

The performance of a small group depends upon the channels of communication open to its members, the task the group must handle, and the stress under which they work.

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INTELLECTUAL PERFORMANCE OF CRYPTOGENIC EPILEPTICS, SYMPTOMATIC EPILEPTICS, AND POSTTRAUMATIC ENCEPHALOPATHS[1]

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TRADITIONALLY, the epileptic individual is reported to show mental deterioration (19). Various explanations have been offered for this deterioration: brain damage suffered prior to the onset of seizures (2, 4), the effect of the seizures (4, 19), and over-sedation (15, 20). Statements concerning deterioration in epilepsy have been based upon subjective impressions of institutionalized epileptics and upon psychological studies which are generally subject to serious criticism.

Psychological studies have commonly utilized one of two approaches: (1) comparison of mean I.Q.'s of institutionalized epileptics, with I.Q. norms of the general population (7, 8, 9), but many of these epileptics were placed in institutions because they were feebleminded as well as epileptic (8, 14); (2) test-retest studies (2, 4) which were largely inconclusive, some authors (23) reporting deterioration in epileptic groups as a whole after an interval of time, others reporting no such change.

There is still a need in this field for studies which control important extraneous variables (age, sex, race, institutionalization, etiology, mental status, type of seizure). It is a major objective of this study to control the pre-epileptic intelligence level.

A number of studies have been devoted to the performance of brain-damaged human beings and animals on tasks of varying difficulty and complexity. The work with brain-damaged rats (17, 18) and monkeys (18) suggest that there is a relationship between the ability to perform complex tasks and the presence or absence of brain damage. Some authors apparently find this same relationship in brain-damaged human beings (10, 11). Brain-damaged patients are more likely to have difficulty in performing at a complex (abstract) level than at a simple (concrete) level.

Since cryptogenic epileptics have suffered no demonstrable brain damage, whereas symptomatic epileptics have incurred demonstrable brain damage, the following hypotheses concerning the relative performance of these groups and selected control groups on specialized tests of intelligence are suggested.

General Hypotheses

I. Symptomatic epileptics show impairment of function when compared with normal subjects.

II. Posttraumatic encephalopaths show impairment of function when compared with normal subjects.

III. Cryptogenic epileptics do not show impairment of function when compared with normal subjects.

IV. If, contrary to hypothesis III, cryptogenic epileptics do show impairment of function when compared with normal subjects, they will still show significantly less impairment than symptomatic epileptics and posttraumatic encephalopaths.

V. Whenever an observed difference is found between either of the brain-damaged groups and either of the non-brain-damaged groups, the direction of the difference will favor the non-brain-damaged group.

Specific Hypothesis

1. Symptomatic epileptics and posttraumatic encephalopaths will show greater im-

[1] This paper is a dissertation which is essentially the same as that submitted in partial fulfillment of the requirements of the degree of Doctor of Philosophy, in the Department of Psychology of the University of Pennsylvania. Published with permission of the Chief Medical Director, Department of Medicine and Surgery, Veterans Administration, who assumes no responsibility for the opinions or conclusions drawn by the author. The writer wishes to express his sincere appreciation to Dr. Miles Murphy, Dr. Mortimer Garrison, Jr., and Dr. James C. Diggory of this University and Dr. Francis Forster, Georgetown University Hospital, for the invaluable assistance and encouragement so generously given throughout the experimental investigation.

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pairment of function: (1) in the formation of new associations than in recalling old associations as measured by the Wechsler Memory Scale Associate Learning Test; (2) on the PMA Reasoning Test than on the PMA Verbal Meaning Test; (3) on the Progressive Matrices Test, Section E, than on the Progressive Matrices Test, Section C.

METHOD

Subjects

1. The *Ss* in this experiment consisted of four groups of white male veterans of World War II. These groups were composed of normals, cryptogenic epileptics, symptomatic epileptics, and posttraumatic encephalopaths, respectively. The epileptic groups had a history of grand mal seizures.

The epileptic classifications are defined by Penfield and Erickson (23) as follows: *symptomatic*—epileptics are classified as symptomatic when a lesion or agent has been found or may be reasonably assumed; *cryptogenic*—epileptics are classified as cryptogenic when no cause can be found or reasonably assumed.

2. There were 15 *Ss* in each of the groups.

3. Members of the normal group consisted of *Ss* who did not have neuropsychiatric diagnoses and had not requested treatment for such conditions. These subjects were selected from personnel at the Philadelphia Naval Hospital, Veterans Administration employees, students attending a meat cutters school, and veterans who volunteered their services when called in for pension examinations. All the *Ss* were volunteers and were selected for the experiment if they could be matched for age, education, and intelligence with *Ss* in the clinical groups. The epileptics and posttraumatic encephalopaths were selected from veterans who had been treated on an outpatient basis by the Veterans Administration in the Philadelphia area.

4. The epileptics reported that they had been free of seizures for a period of at least 24 hours prior to testing, so that the test results were not likely to be influenced by the after-effects of a seizure. None of the epileptics reported having a seizure in the 24-hour interval after testing.

5. No *S* suffering from a gross sensory or motor defect was used.

6. The epileptics were classified as to type and etiology by a neurologist. The posttraumatic encephalopaths were also diagnosed by a neurologist. All the *Ss* in the clinical groups had been observed, studied, and treated in a hospital at some time in the past. Diagnoses were based upon the clinical history, a physical examination, and a neurological examination. In addition, most of the *Ss* had had one or more electroencephalograms and skull X-rays. Laboratory studies such as lumbar punctures, pneumoencephalograms, eye studies, blood studies, etc., had also been done when indicated.

7. The individuals in each of the groups were matched with individuals in each of the other groups for age, education, and intelligence. The two epileptic groups were matched for type of seizures and length of time subject to seizures. The interval of time between the occurrence of the brain damage and the psychological examination was approximately the same for the symptomatic epileptics and the posttraumatic encephalopaths.

It was most important in this experiment to control in the experimental groups the level of intelligence prior to the onset of the epilepsy and prior to the occurrence of the brain damage. If it is known that the groups were formerly of equal intelligence, and if differences are now found in performance on the tests, it can be logically inferred that the intellectual impairment is related to the variables being investigated and not to some other uncontrolled factors. Therefore, the *Ss* in the groups were matched upon the basis of the General Classification Test scores attained at the time they entered the service, or upon the results of intelligence tests obtained from the schools they had attended prior to entering the service. When these foregoing measures could not be obtained, results from intelligence tests given after the occurrence of the brain damage or the onset of the epilepsy were utilized for matching purposes. Such cases were equally distributed throughout the four groups. It is recognized that these intelligence test results in the clinical groups might be affected adversely by the epilepsy or the brain damage. If they were affected adversely, then this would minimize the amount of intellectual impairment in the clinical groups and work against the hypotheses. Consequently, it can be assumed that the obtained differences are minimum estimates.

The various intelligence test scores were converted into standard scores (*Z*) with a mean of 100 and a standard deviation of 10. The *Ss* were then matched for intelligence on the basis of these standard scores.

The matching data for the four groups are presented in Table 1.

8. There was no systematic attempt to control the kind or amount of medication given to the epileptic groups. There is sufficient evidence in the literature (23) to indicate that medication does not result in impaired intellectual functioning.

Selection of Tests

In a review of the literature on psychological changes following organic brain lesions and ablation, Klebanoff (16) points out that the conventional or gross tests of general intelligence and performance ability are not sensitive enough to measure functional losses which occur as a consequence of brain damage. This statement was based upon the results of psychological examinations before and after the removal of brain tissue. Studies by Goldstein (10), Armitage (3), Bender (5), Capps (6), and others, demonstrate the value of using specialized tests for the investigation of psychological functions in patients with disease or injury of the brain. Specialized tests are those tests which are

TABLE 1

MATCHING DATA

VARIABLES	GROUP *			
	N	C	S	P
Number of subjects	15	15	15	15
Age, mean	26.96	26.63	27.63	27.23
Age, range	22.75–31.83	23.42–31.25	23.42–34.83	22.33–32.08
Education, mean years	12.00	12.07	11.80	12.00
Education, range	9–16	10–17	9–16	8–16
Intelligence (*Z* scores), mean	108.00	108.13	107.53	107.66
Intelligence (*Z* scores), range	94–120	95–121	96–121	100–122
Years since onset of seizures, mean		5.09	4.65	
Years since onset of seizures, range		3.58–8.33	3.50–6.00	
Years since cerebral injuries, mean			5.28	5.15
Years since cerebral injuries, range			3.75–6.17	1.75–6.75

* The following symbols, also used in subsequent tables, refer to the different groups:
N—Normals
C—Cryptogenic epileptics
S—Symptomatic epileptics
P—Posttraumatic encephalopaths
There were no significant differences between any of the groups on any of the variables.

utilized to measure specific intellectual defects, such as defects in the ability to think abstractly, defects in the attentional sphere, defects of memory, etc. Consequently, specialized tests, which are believed to be of value in detecting areas of intellectual impairment in patients with disease or injury of the brain, were used in the experiment.

The following tests were selected because previous studies (1, 3, 5, 6, 8, 10, 16) have demonstrated that these tests, or similar ones, have proved useful in investigating areas of impairment in patients with disease or injury of the brain.

1. *Verbal Meaning* (SRA Primary Mental Abilities)
2. *Space* (SRA Primary Mental Abilities)
3. *Reasoning* (SRA Primary Mental Abilities)
4. *Digit Symbol* (Wechsler-Bellevue, Form II)
5. *Associate Learning* (Wechsler Memory Scale)
6. *Progressive Matrices Tests* (Sets C, D, and E).

In order to decrease the length of time to administer the test battery and because of their apparent simplicity, sets A and B were not administered.

Procedures

1. The tests were administered individually in a single testing session by the author in the order given above. The testing sessions lasted approximately one hour. The administration and scoring were based upon published directions.

2. Every group was compared with each of the other groups for performance on each of the tests. In addition to these comparisons, the normal group and the cryptogenic group were combined into a group which is called a "non-brain-damaged group" and compared with a group composed of the symp-

TABLE 2

OBTAINED MEANS * AND SIGNIFICANCE OF THE DIFFERENCES BETWEEN MEAN TEST SCORES EXPRESSED AS PROBABILITIES BASED UPON *t*-TESTS

TEST	GROUP			GROUP		
	N	C	P_{att}	S	P	P_{att}
PMA Verbal Meaning	36.47	35.73	.80–.90	26.00	28.47	.50–.60
PMA Space	28.87	30.53	.60–.70	18.33	21.93	.20–.30
PMA Reasoning	16.87	17.93	.60–.70	8.80	10.80	.40–.50
WB II Digit Symbol	52.93	54.73	.50–.60	47.07	42.80	.20–.30
WMS Associate Learning, total	16.53	16.90	.70–.80	12.93	13.50	.60–.70
WMS Associate Learning, easy	17.33	16.87	.20–.30	16.00	16.33	.50–.60
WMS Associate Learning, hard	7.87	8.47	.50–.60	4.93	5.33	.60–.70
Progressive Matrices (Sets C, D, and E)	23.73	22.87	.60–.70	18.60	18.40	.90–1.00
Progressive Matrices, Set C	8.53	7.60	.20–.30	7.07	7.27	.80–.90
Progressive Matrices, Set D	9.00	8.53	.40–.50	7.47	7.00	.60–.70
Progressive Matrices, Set E	6.20	6.80	.50–.60	4.07	4.13	.60–.70

* All the scores are mean raw scores with the exception of the WMS Associate Learning total, which is expressed as a weighted score based upon the formula given by Wechsler.

tomatic epileptics and posttraumatic encephalopaths. This latter group is called the "brain-damaged group."

In order to test the differential impairment of simple versus complex functions in the symptomatic epileptics and posttraumatic encephalopaths, the raw scores of the tests investigated were converted into standard scores.

3. Critical ratios (t-tests) were computed to determine the significance of the obtained difference.

RESULTS AND DISCUSSION

Table 2 gives the mean test scores of the four groups and the significance of the differences between the normal and the cryptogenic groups, and between the symptomatic and posttraumatic groups. The mean test scores of the normal and cryptogenic groups are essentially the same, and there are no significant differences between these groups on any of the tests. The hypothesis that cryptogenic epileptics do not show impairment of function on specialized tests of intelligence appears confirmed by these data.

The mean test scores of the symptomatic and posttraumatic groups are essentially the same, and there are no significant differences between either of these two groups on any of the tests. This would seem to indicate that the presence of an epileptic condition in posttraumatic encephalopaths does not result in a greater degree of impairment of intellectual functioning.

Since there were no significant differences in performance between the two non-brain-damaged groups or between the two brain-damaged groups, the normal and cryptogenic groups were combined to form the non-brain-damaged group, and the symptomatic and posttraumatic groups were combined to form the brain-damaged group. The mean test scores and P values of these groups appear in Table 3. For total test scores, the P values are all less than .01 in favor of the non-brain-damaged group. Significant differences are also found on both parts of the WMS Associate Learning Test and on Sets D and E of the Progressive Matrices Test. The hypotheses that symptomatic epileptics and posttraumatic encephalopaths show impairment of function on specialized tests of intelligence when compared with normal *S*s and cryptogenic *S*s appear to be confirmed by the above data.

TABLE 3

OBTAINED MEANS AND SIGNIFICANCE OF THE DIFFERENCES BETWEEN MEAN TEST SCORES EXPRESSED AS PROBABILITIES BASED UPON t-TESTS

TEST	NON-BRAIN-DAMAGED	BRAIN-DAMAGED	P
PMA Verbal Meaning	36.10	27.23	<.01
PMA Space	29.70	20.13	<.01
PMA Reasoning	17.40	9.80	<.01
WB II Digit Symbol	53.83	44.93	<.01
WMS Associate Learning, total	16.72	13.22	<.01
WMS Associate Learning, easy	17.10	16.17	<.02
WMS Associate Learning, hard	8.17	5.13	<.01
Progressive Matrices (Sets C, D, and E)	23.30	18.50	<.01
Progressive Matrices, Set C	8.07	7.17	.10–.20
Progressive Matrices, Set D	8.77	7.23	<.01
Progressive Matrices, Set E	6.50	4.10	<.01

Since there is no intellectual impairment in the cryptogenic epileptics as measured by the tests used in this investigation, and since symptomatic epileptics show impairment of function on these same tests, it seems reasonable to conclude that it is the presence of the brain damage, not the epilepsy, that causes the impairment.

A breakdown of the WMS Associate Learning Test into easy and hard items reveals that lower P values are present on the latter than on the former when the non-brain-damaged groups are compared with the brain-damaged groups (see Table 4). This same trend is also apparent when the PMA Verbal Meaning Test is compared with the PMA Reasoning Test, and when Sets C and E of the Progressive Matrices Test are compared. Although significant differences were not found when testing for differential im-

pairment of simple versus complex functions in the symptomatic and posttraumatic groups, in every case the derived t value was greater on the tests of the more complex functions than on the tests of the less complex functions. This same relationship was also found when the two groups were combined into a single brain-damaged group. Such an outcome, expressed as a probability ratio based upon a binomial expansion, would occur by chance less than two times in a hundred. This would support the point of view that there is greater impairment of complex functions than of the less complex functions in the brain-damaged groups.

It has been reported (15) that intellectual impairment in epileptics may be a consequence of the medication used to control seizures and that the longer the history of seizures, the greater the impairment. An analysis of the data in the present study does not support this point of view. There were no significant rank order correlations between test performance and the length of time since the onset of seizures in the cryptogenic group. Inasmuch as the cryptogenic epileptics were on medication and no impairment was found, it seems likely that neither medication nor the length of time subject to seizures results in impaired intellectual efficiency. A follow-up study after a lapse of several years would help to clarify these problems also.

TABLE 4

THE BODY OF THIS TABLE CONTAINS THE t VALUES OBTAINED BY COMPARING THE BRAIN-DAMAGED GROUPS WITH THE NORMAL GROUP ON THE INDICATED TESTS *

TEST	$t_{Diff\ N-S}$	$t_{Diff\ N-P}$	$t_{Diff\ NBD-BD}$ †
PMA Verbal Meaning	2.933	2.022	3.094
PMA Reasoning	4.055	2.379	4.516
WMS Associate Learning, easy	2.428	1.931	2.548
WMS Associate Learning, hard	3.763	2.530	4.620
Progressive Matrices, Set C	1.895	1.620	1.453
Progressive Matrices, Set E	2.235	1.865	3.089

* There were no significant differences when testing for differential impairment of simple versus complex functions in the symptomatic and posttraumatic groups.
† NBD—Normal subjects and cryptogenic epileptics combined into a non-brain-damaged group.
BD—Symptomatic epileptics and posttraumatic encephalopaths combined into a brain damaged group.

It was felt that the data might be investigated further in order to throw light on certain other problems which have arisen in connection with epileptics and organics. Having demonstrated the presence of intellectual impairment in the brain-damaged group, the question arises whether there is any relationship between the degree of impairment and the interval since the brain damage occurred. Rank order correlations were computed between test performance and length of history of brain damage. All but one of these correlations were positive and, of the positive correlations, only one was significant at the .05 level of confidence. Conclusions pertaining to the nature of the relationship between test performance and length of history of brain damage based upon such low correlations are not warranted and must be held in abeyance until more conclusive evidence can be obtained by follow-up studies several years hence.

It is generally believed that scores on vocabulary tests "hold up" better than tests which involve other types of material. The findings of the present study do not support this point of view. Marked impairment is demonstrated by the mean vocabulary test score of the brain-damaged group. On the basis of this finding, it would be a mistake to use this type of vocabulary test to estimate previous level of intellectual functioning. It is believed that the Binet and Wechsler vocabulary scores "hold up" because the tests are not timed and there is a wide range of acceptable answers.

It is interesting to note that there were no reversals of any of the symbols on the Digit Symbol Test. Reversal of symbols is generally accepted as being indicative of an organic condition, but the data in the present experiment would lead to the conclusion that the absence of such reversals does not elimi-

nate the possibility of an organic condition.

An analysis of the test data indicated that the brain-damaged *Ss* had more difficulty in stopping a given task when they were told to stop than the non-brain-damaged *Ss*. There were 17 occasions, involving 13 of the brain-damaged *Ss*, when the *Ss* made entries on the test blanks after they were told to stop. This same sort of behavior occurred eight times, involving eight of the non-brain-damaged *Ss*. The difference between the two groups was subjected to a *t*-test, which yielded a *P* value that lies between the .05 and .10 level of confidence. There is a possibility that this difference may be due to a difference in motivation. Knowing that they have brain damage, these patients may try harder to prove to themselves and to the examiner that they can perform well on the psychological tests. Being highly motivated to make a good impression, they find it difficult to stop the instant they are told to do so. This behavior might also be interpreted as perseverative behavior, which is reported (15, 23) to be characteristic of brain-damaged patients. No definite conclusions can be drawn on the basis of this evidence, and an adequate explanation will have to be postponed until further research can be carried out in this area.

The question arises as to whether other tests of intelligence would have yielded the same results if they had been used in this experiment. Even though the tests in this experiment were selected on the basis of previous studies of brain-injured people, it seems very likely that impairment would have been demonstrated regardless of the tests which might have been used. This line of reasoning is based upon an analysis of the different tests utilized in the present battery. Differences were found on the easy tests as well as on the hard tests. There are 44 possible comparisons of a brain-damaged group and a non-brain-damaged group on both total test scores and subtest scores. In most instances the non-brain-damaged group did significantly better than the brain-damaged group. In every instance the direction of the difference favored the non-brain-damaged group.

Although Verbal Meaning, Space, and Reasoning were treated as separate specialized tests, they are actually three of the five subtests which make up the SRA Primary Mental Abilities Test. Since differences were found with these three tests, it seems likely that impaired functioning would have been apparent in the total score as well. This type of test is fairly representative of the more conventional paper and pencil tests which are in usage today. The Progressive Matrices is a nonverbal test of intelligence which is not timed. Total test scores on this test also demonstrate impairment of intellectual functioning. Thus it would seem that in so far as the tests in this experiment are representative of tests of intelligence, impaired functioning would most likely have been found on any of the tests that might have been used.

The results of this experiment demonstrate rather clearly that psychological impairment can be detected with objective tests and that we do not have to rely completely upon qualitative measures of psychological impairment when testing organics. It is believed that other experiments failed to show conclusive evidence of impairment because of their failure to control the necessary variables, especially the pretrauma level of intelligence.

There have been a number of studies which have attempted to relate psychological impairment to specific areas of the brain. The results are somewhat equivocal. Although some of the studies (16) report such relationships, the extensive and systematic investigation of the Columbia-Greystone Associates (21) failed to demonstrate with an exhaustive test battery permanent impairment of mental function in a group of psychotic patients after removal of specified anatomical areas from the frontal lobes. This latter investigation, as well as others of this type, compared intellectual functioning under different abnormal conditions instead of comparing normals with organics. These studies generally compared performance before and after the removal of brain tumors, before and after lobotomies of psychotics, etc. It would appear more important and certainly more fundamental to investigate first the more obvious differences between normals and organics. This would enable us better to understand the differences in test performance in different forms of brain disorders.

Harrower-Erickson (12) and Hebb and Penfield (13) have argued that the excessive

deficit found in the organic disorders and commonly attributed to cerebral damage, is more likely due to the presence of pathological tissue than the absence of brain tissue. If true, it may partially account for the failure of the Greystone studies to find impairment. The data of the present study certainly demonstrate that the presence of pathological tissue results in impairment, but nothing can be said about the absence of brain tissue.

Wechsler (24) supports the theory that the brain acts as a whole and its impairment in one place affects the functioning as a whole. This theory of generalized cerebral function is supported by Lashley's work with brain-damaged rats. He reported a reduction in the ability of rats to learn maze habits regardless of the location of the lesion on the cortex. In an attempt to throw some light on this problem, the records of the 30 brain-damaged subjects in the present study were reviewed with the idea of dividing them into two groups depending upon whether the damage was anterior or posterior to the Rolandic fissure. A study of the records revealed that no such localization could be made with confidence. It was next thought that the brain damage might be localized in either the right or the left cerebral hemisphere. Upon reviewing the records, nine pairs of cases were found in which one member of the pair had suffered brain damage in the left hemisphere and the other member of the pair had suffered brain damage in the right hemisphere. These pairs could furthermore be matched with respect to intelligence, age, and education. These nine matched pairs were then studied, but no significant differences were discovered. Likewise, the test results did not indicate any consistent trend for the group with brain damage in the right cerebral hemisphere to do better than the group with brain damage in the left cerebral hemisphere. These results are interpreted as tentative evidence for a theory of generalized cerebral function, but no conclusions can be drawn because of the small number of cases.

There seems to be a need for research with normal people before and after the removal of brain tissue. Such a procedure of course is impossible, but a recent trend in the treatment of psychomotor epileptics by surgical removal of the temporal lobe focus, which is believed to be responsible for the psychomotor seizures, presents an excellent opportunity to clarify findings in this area. Admittedly, these epileptics are not normal in the strictest sense of the word, but they are probably the most normal people available who are likely to have surgical removal of brain tissue. Until such work can be done, perhaps the next best line of approach would be to extend the experimental design developed in this experiment to a study of other organic groups and to a study of cryptogenic epileptics whose seizures began early in life. A study of the latter group should prove to be interesting in that seizures may interfere with mental growth. It is conceivable that such interference might take place because the brain is not as well protected and is more sensitive to injury early in life. A longitudinal study of such a group would be extremely valuable.

CONCLUSIONS

1. Cryptogenic epileptics do not show impairment of function on specialized tests of intelligence.

2. Symptomatic epileptics show impairment of function on specialized tests of intelligence.

3. Posttraumatic encephalopaths show impairment of function on specialized tests of intelligence.

4. Symptomatic epileptics and posttraumatic encephalopaths do not differ significantly on specialized tests of intelligence.

5. The above findings are interpreted as supporting the point of view that it is the presence of the brain damage and not the presence of the epileptic condition which is responsible for impaired intellectual functioning.

6. There is suggestive evidence to support the point of view that there is greater impairment of complex functions than of the less complex functions in the brain-damaged groups.

7. Low positive correlations, with only one significant at the .05 level of confidence, were found between test performance and length of history of brain damage. This evidence was considered insufficient for evaluating the dependence of the degree of impairment upon length of history of brain damage.

8. There is no evidence to indicate that either length of time since the onset of

seizures or the medication used to control the seizures results in impaired intellectual efficiency.

9. Impaired intellectual functioning on the Verbal Meaning Test is interpreted to preclude the usage of similar vocabulary tests to estimate a previous level of intellectual functioning.

10. There were no reversals of any of the symbols on the Digit Symbol Test, which is contrary to the general clinical belief that such reversals are characteristic of the performance of brain-damaged patients.

11. A tendency for the brain-damaged group to continue a task after being told to stop is interpreted as being suggestive of perseverative behavior or as being indicative of better motivation.

12. Objective tests are sensitive enough to measure psychological impairment in brain-damaged patients.

13. It is believed that other tests of intelligence would have yielded similar results if they had been used in this experiment.

14. Brain damage in the left cerebral hemisphere did not produce any greater impairment than brain damage in the right cerebral hemisphere. This is interpreted as being suggestive evidence for a theory of generalized cerebral function.

15. Future research should extend the experimental design developed in this experiment to a study of other organic groups, psychomotor epileptics, and cryptogenic epileptics whose seizures began early in life.

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EXPERIMENTAL STRESS AND ALLEGED RORSCHACH INDICES OF ANXIETY[1]

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SOME of the most important and frequently utilized Rorschach signs in need of experimental investigation are those alleged to indicate anxiety. While support for these signs derives mainly from clinical sources (2, 11, 18) there are clinical studies which fail to corroborate the allegations (25, 26). A review of the literature in this area also reveals that there has been no adequate experimental study of these indicators (3). This study is, therefore, offered as one possible approach for investigating the problem experimentally.

PROBLEM

The present experiment is concerned with the investigation of the influence of a stress-produced anxiety on the occurrence of Rorschach factors alleged to be signs of anxiety. The influence of this stress-produced anxiety was determined by comparing the Rorschach performance of two matched groups of *Ss*, one of which took the test under stress, the other under standard conditions, with specific reference to 15 test indices commonly regarded by Rorschach experts (2, 11, 18, 27) as anxiety indicators. An over-all judgment of the extent of anxiety shown in the protocols was also obtained by having two experienced Rorschach interpreters rate each test record for the degree of anxiety exhibited. As independent gauges of the stress-produced anxiety, two additional indices were employed: efficiency of performance on a subtraction task and self-ratings on an anxiety-tension scale.

The following testable hypotheses relating to the performance of the two differentially treated groups of *Ss* on the 15 alleged Rorschach anxiety measures were formulated:

a) The total number of responses (*R*) to all cards will be *less* for the stress than for the control group.

b) Total number of card rejections (*Rej*) will be *greater* for the stress than for the control group.

c) The total number of responses to the whole card (*W*) for all cards will be *less* for the stress than for the control group.

d) The total number of oligophrenic details (*Do*) for all cards will be *greater* for the stress than for the control group.

e) The total number of rare details (*Dr*) for all cards will be *greater* for the stress than for the control group.

f) The total number of responses for all cards using human movement (*M*) as a determinant will be *less* for the stress than for the control group.

g) The total number of weighted shading responses[2] (*FSh*=.5, *ShF*=1.0, *Sh*=1.5) for all cards will be *greater* for the stress than for the control group.

h) The total number of responses for all cards using form (*F*) as sole determinant will be *greater* for the stress than for the control group.

i) The total number of weighted color responses (*FC*=.5, *CF*=1.0, *C*=1.5) will be *less* for the stress than for the control group.

j) The total number of animal forms (*sum A*) for all cards will be *greater* for the stress than for the control group.

k) The total number of parts of animal forms (*Ad*) for all cards will be *greater* for the stress than for the control group.

[1] A modification of a portion of a thesis submitted to the Department of Psychology of the State University of Iowa in partial fulfillment of the requirements for the Ph.D. degree. The writer is indebted to Professor Arthur L. Benton under whose direction this investigation was undertaken. Acknowledgment is also made of the generous assistance of Professors Harold P. Bechtoldt and Woodrow W. Morris.

• Now at the New York Regional Office of the Veterans Administration.

[2] Following Hertz (9) these include responses determined by the achromatic colors except white.

l) The total number of parts of human forms (*Hd*) for all cards will be *greater* for the stress than for the control group.

m) The total number of anatomical forms (*Anat*) for all cards will be *greater* for the stress than for the control group.

n) The total number of popular responses (*P*) for all cards will be *less* for the stress than for the control group.

o) The total number of shading shock signs[3] (*S.S.*) will be *greater* for the stress than for the control group.

METHOD

Subjects

Sixty-nine male students enrolled in various courses in psychology and speech pathology were originally run in the experiment. Sixty of these could be matched on certain scores on the Behn-Rorschach test as well as on other variables (to be discussed subsequently) and were used as the *S*s for this experiment.[4] Thirty of the *S*s served in the experimental (stress) group and thirty in the control. Four of the nine unmatched *S*s were run in the stress group as possible alternates or reserves; the remaining five were assigned to the control group, their records to be used in an investigation not directly related to the present study. None of the *S*s admitted having any previous experience with the Rorschach test. All appeared to be unaware of the specific nature and purpose of the experiment.

Apparatus

One of the important conditions defining the stress situation was the administration of electric shocks. The apparatus used for this purpose consisted of a General Radio variac, two 6.3-volt filament transformers, two sponge rubber electrodes, each 5/8 in. in diameter and 3/16 in. thick, and switches for the shock application. The primary source current was 110 A.C. line current which was reduced by the variac to the desired voltage. The sponge rubber electrodes were thoroughly moistened in saline solution and attached to *S*'s left cheek and wrist with strips of adhesive tape. All of the actual apparatus for shock administration was screened from *S*'s view.

Visible to the *S* was a panel, on which were mounted a number of binding posts, a fuse, an "off-on" switch, and three variable knobs labeled "motor," "lamp," and "intensity." The binding posts under the heading "high resistance" were labeled "ohms" and numbered from 50 to 1500. To the side of this panel a double-throw five-pole knife switch was prominently displayed. A group of insulated wires ran from the switch to the panel and appeared to be connected with the rest of the apparatus by wires leading behind the screen. Actually the switch and panel were not connected to any electrical source and were used solely for their suggestive effects.

The *S* sat in a chair directly to the side of a table on which the aforementioned pieces of electrical equipment were placed. In appearance and construction the chair suggested an "electric chair." A wide seat and high back were made of black insulating fiber board. Attached to the front of either arm was a small aluminum globe. A web belt 1 in. wide was fastened over the arms of the chair, in safety-belt fashion, after *S* was seated. Two shorter belts of similar width were used to fasten the *S*'s legs to the front legs of the chair.

Also used for their suggestive effects were two dummy silver electrodes, the size of a dime and quarter respectively, a helmet, and an electric phonograph motor. The metal electrodes were attached to *S*'s right cheek and wrist with strips of adhesive tape. The helmet consisted of a web belt to which was attached one large metal strip (extending over the subject's forehead) and two 1-in. metal squares (one on each side of *S*'s head). Wires from the silver electrodes and the two metal squares ran toward the five-pole switch and appeared to be connected to the wires leading behind the screen. The phonograph motor, concealed behind the screen, was used to provide a continuous hum, just barely audible, suggesting to *S* that the complex electrical equipment was in operation.

Procedure

The entire experiment was divided into two sessions. During the first session all apparatus was

[3] There are a considerable number of signs of shading shock. Most of these signs, however, are described in such a general and vague manner that objective and reliable application of them is practically impossible. A method of quantifying the indices of shading shock was devised by using the signs listed below. The total shading shock score for each individual was obtained by adding the number of signs appearing on each of Cards 4, 6, and 7. The following are the signs employed:

a) Rejection of card.
b) Delay equal to or greater than mean initial response time (based on all cards) plus one average deviation.
c) Number of responses less than one-half the mean number of responses to all cards.
d) Increase in *F*— responses equal or greater than the mean number of such responses (based on cards where the determinant appears) plus one average deviation.
e) Rare detail as first response.
f) Oligophrenic detail as first response.
g) Absence of *P* or appearance delayed beyond second response.
h) Twice the mean number of animal responses on all cards.
i) Twice the mean number of anatomical responses on all cards.
j) Appearance of *M*— responses.
k) Verbalization of dislike of card.

[4] Of these 60 students, 53 were attending the introductory psychology course, seven (education or speech majors) were attending courses in speech pathology, educational psychology, or behavior disorders in children.

hidden from view. The Behn-Rorschach[5] was administered according to the procedure outlined by Beck (1) for administering the Rorschach test, with the exception that a card was withdrawn with the remark, "All right, let us go on to the next one," after ten responses had been given to it. The test was scored using Hertz's (7) scoring symbols. In computing the scores for each of the symbols used, each main response was counted as 1 and each additional scoring counted as 0.5.[6]

Two groups of *S*s were then formed by matching *S*s individually on the following five items:

a) Number of responses.
b) Sum of weighted shading responses.
c) Presence or absence of card rejections.
d) Presence or absence of time delay on shading cards (delay defined as initial reaction time longer than the mean initial reaction time plus one average deviation).
e) Experience balance category (*constricted* type—number of *M* responses, one or less, and sum of weighted color responses, one or less; *dilated* type—number of *M* responses, four or more, and sum of weighted color responses, four or more; *average* type—that *M*/*sum C* ratio not falling in the constricted or dilated categories).

The two groups were also equated in respect to the following group means:

a) Number of human movement responses.
b) Number of weighted color responses.
c) Age.
d) College year.
e) Cumulative grade point average.

The groups were then randomly assigned to the stress or control conditions.

The median time interval elapsing between the first and second sessions for the experimental (stress) group was 19 days, with a range of 11 to 36 days. For the control (non-stress) group, the median time interval was 20 days, with a range of 11 to 37 days.

Stress group. Upon entering the room for the second session each *S* in the stress group was seated in the "electric chair" and the two sponge electrodes were applied to the upper left cheek and the undersurface of the left wrist, respectively. The following statement was then made:

> You are participating in a physiological psychology experiment which is concerned with investigating the relationship between perception and bodily resistance to electricity. Before we deal with the perceptual aspects of the experiment, I would like you to do a simple subtraction task for me.
>
> I want you to start from number 750 and subtract an increasing series of numbers. The series of numbers runs from 1 through 25, increasing by 1 each time. For instance, from 750 you will subtract 1, from 749 subtract 2, from 747 subtract 3, from 744 subtract 4, and so on, until you have subtracted an amount of 25. When you have subtracted an amount of 25, the next number you will subtract *from the particular remainder you have*—do not go back to 750—will be 1 again, than 2, then 3, and so on. In other words, you will never subtract an amount greater than 25, but will subtract amounts running from 1 through 25 only. You will continue subtracting, always reducing your remainder, until you reach 0 or a negative number, whichever it turns out to be.
>
> Here is an example partly worked out, assuming we had started from 1000. [At this point, an example showing the subtraction process for one series and part of another was exhibited.] From 1000 we subtract 1, from 999 we subtract 2, from 997 we subtract 3 and so on down; [pause] from 724 we subtract 24, from 700 we subtract 25, from 675 we subtract 1 again, from 674 subtract 2, thus increasing the series again. You will follow the same pattern except that you will start from 750. Remember to continue to subtract until you reach an amount of 0 or your first negative number, then stop.
>
> This task is *not* a speed test. Please start when I tell you to. Do you have any questions? [At this point *S* was given a sheet of unlined paper, a pencil, and a board on which to write.]
>
> While you are subtracting you will receive shock. The shock will come at random intervals and may occur at one or both electrode points. Each successive shock will be somewhat more intense than the preceding one; I do not have anything to do with the presentation of the shock. The shock is automatically administered by the machine.

Certain "adjustments" of the panel knobs were made and the five-pole knife switch was closed simultaneously with the activation of the phonograph motor. The subject was then told: "You can start." The *E* at this point was seated at the table to the back and right of the *S*. In this position it was possible to observe *S* and also operate the necessary apparatus for delivery of shock.

Shock was administered at the end of 1 minute, 2 minutes, and at the point where *S* had just reached a remainder in the 100's (if the subtraction was performed accurately the number was 194). The three shock volts were 25, 30, and 35, respectively. Duration of shock was a fraction of 2 second.

When *S* had finished, the paper was removed and the time required for subtraction recorded. The five-pole knife switch was opened simultaneously with the stopping of the electric motor. Two metal electrodes were then attached to *S*'s right wrist and upper right cheek, respectively. One web belt was fastened over the arms of the chair while *S*'s legs were fastened by two other belts to the front legs of the chair. A helmet was then adjusted on *S*'s head.[7] The *S* was then given another sheet of paper and the following statements were made:

> The next part of the experiment is rather crucial; therefore I would like you to write the following as I dictate it to you: "I do hereby pledge that I will not

[5] This test was employed to minimize the influence of memory on the standard Rorschach test administered during the second session. It was felt that previous experience with the Rorschach might mask the influence of the stress conditions. The possible distorting effects of previous experience with the Rorschach on retest performance was recognized by Rorschach himself (19) and has been noted by many subsequent investigators (6, 22, 23, 24).

[6] Since no published norms for the Behn exist, scores for *P*, *Do*, *Dr*, and form quality were not used.

[7] Up to this point, none of the additional equipment (metal electrodes, web belts, and helmet) had been seen by the *S*.

reveal the nature of any part of this experiment to any individual." Sign your name underneath.

The paper was removed and the following statements were made:

We shall now go through a set of cards somewhat similar to the ones you saw last time, following the same procedure. While you are going through these cards, you will again receive shock. The shock will again come at random intervals at one or all places where the electrodes are located. Again, each succeeding shock will be somewhat more intense than the preceding one. *The longer the time interval that elapses without the receipt of shock, the more intense will the next shock be.*

The five-pole knife switch was closed simultaneously with the reactivation of the motor. The *E* again sat to the right and in back of *S*, and the Rorschach test was administered following the same procedure used with the Behn. *No shock was given during the Rorschach test.*

Upon completion of the administration of the Rorschach the switch was opened and the motor stopped. The electrodes and belts were removed and *S* was asked to relax in another chair near *E*'s desk. The following statement was then made:

This investigation is still more or less in the exploratory stage. It is felt that the experimental design used here may have produced certain feelings in the subject and that these feelings may be influencing the results we are getting. Here is an anxiety or tension scale. Would you place a line through this scale indicating in general how you felt while doing the subtraction task. You may put your line at any point that you feel is the appropriate one; it need not be directly on a number. Would you also put another line through the scale indicating in general how you felt while going through the ten cards for the first time, today.

The following seven-point scale was used:

0	1	2	3	4	5	6
Completely relaxed, no anxiety or tension			Moderately anxious or tense		Extremely anxious or tense	

Before leaving the experimental room, *S* was reassured as to his performance on the required tasks and thanked for his cooperation. The importance of not divulging the nature of the experiment was again stressed.

Non-stress group. The second session for the control group was conducted in another room, in which no apparatus was present. The following statements were made:

This session will be quite like the first one. I have here a set of cards somewhat similar to the ones you saw last time and we shall follow the same procedure in going through them. However, before we go through this perception task, I want you to do a simple subtraction task for me.

(The instructions for the subtraction task utilized for the stress group were given, beginning with "I want you to start from . . .," and ending with ". . . then stop.")

The following statements were added:

The task is quite simple. It is not a speed test. Do you have any questions you want to ask? O.K., you can start at any time.

Upon completion of the subtraction task, the paper was taken by *E*, and the time required for the test's completion was noted. The Rorschach test was then administered in the manner indicated previously. Upon completion of the Rorschach administration, the anxiety-tension scale was presented with the same instructions used for the stress group.

RESULTS

Matching Procedure

The means and standard deviations on seven of the ten items on which the stress and non-stress groups were matched before differential treatment are shown in Table 1. As can be seen, the matching on these items was close. The three remaining categories on which the individuals were matched were: presence or absence of card rejections, presence or absence of time delay on shading cards (other than Card I), and type of experience balance. Two individuals in each group rejected Behn cards (a total of three cards were rejected in the stress group, two cards in the non-stress group). Twenty individuals in each group showed a delayed initial reaction time on shading cards. Two individuals in each group had a *dilated* experience balance, while 28 in each group had an *average* experience balance.

Since the Behn-Rorschach was used in the

TABLE 1

COMPARISON OF MEANS AND SIGMAS ON SEVEN OF THE TEN ITEMS USED IN MATCHING

ITEM	GROUP	MEAN	S.D.
*R**	Stress	29.06	15.88
	Non-stress	29.16	16.56
*Sh wt.**	Stress	4.35	3.56
	Non-stress	4.45	3.85
*M**	Stress	2.03	1.70
	Non-stress	1.90	1.54
*C wt.**	Stress	3.78	3.31
	Non-stress	3.84	2.03
Age	Stress	24.06	3.92
	Non-stress	24.10	4.12
College Year	Stress	3.43	1.17
	Non-stress	3.26	1.26
Grade Point	Stress	2.44	0.42
	Non-stress	2.54	0.48

* Behn-Rorschach scores.

original matching to avoid the influence of memory in a test-retest procedure involving the standard Rorschach blots, it is pertinent to ask what the relationship is between the Behn and Rorschach for the matched items. To investigate this, correlation coefficients were computed between the Behn and Rorschach for the 30 individuals in the control group on each of the four scoring symbols used in matching. Rank-difference correlations of .78 and .68 were obtained for number of responses (*R*) and shading weight (*Sh wt.*), respectively; product-moment correlations [8] of .57 and .65 were obtained for human movement (*M*) and color weight (*C wt.*), respectively. All coefficients are significant beyond the 1 per cent level of confidence. The results are in line with the findings of previous investigators (14, 20, 22) and indicate that there is a significant relationship between the two tests for these factors.

A comparison between the tests was also made on the remaining items used in matching. Experience balance categories were quite stable. Of 28 individuals falling in the *average* category on the Behn, 26 remained in that category on the Rorschach, two shifting to a *dilated* experience balance. The two individuals in the *dilated* category on the Behn shifted to an *average* experience type on the Rorschach. Consistency with regard to presence or absence of card rejections was good. The two individuals who rejected cards on the Behn also rejected cards on the Rorschach, 22 individuals did not reject cards on either test, while six individuals who did not reject cards on the Behn did so on the Rorschach. A much lower relationship was found on the comparison of presence or absence of shading delay. Twelve individuals showed delayed initial reaction time on shading cards on both tests, two did not show shading delay on either test, eight showed shading delay on the Behn but not on the Rorschach, while eight showed shading delay on the Rorschach but not on the Behn.

Effects of Stress

Subtraction test. This test is somewhat similar to the task of "subtracting 7's" frequently used by psychiatrists in their mental status examinations. Since it is commonly accepted by clinicians that one of the effects of anxiety is an impairment of intellectual efficiency, it was felt that performance on the subtraction task might furnish a measure (admittedly gross) of the effects of a stress-produced anxiety.

The significance of the difference between the means of the time scores (384 and 403 seconds were required by the stress and control groups, respectively) was tested by means of the *t*-test for related measures and found to be non-significant (t=.59 for 29 df). A similar t for the difference between the mean number of errors (3.00 for the stress compared with 1.86 errors for the control group) was significant between the 2 and 5 per cent levels of confidence (t=2.35 for 29 df).

Possibly another indication of the effects of the stress-produced anxiety is the fact that no one in the non-stress group failed to follow directions, while four people in the experimental group did, by subtracting amounts greater than 25 or continuing their subtraction much beyond the first negative number. However, the limited number of cases makes this finding, at most, a suggestive one.

Self-ratings. The difference between mean self-ratings on the anxiety-tension scale (3.55 for the stress compared with 1.55 for the control group) for the subtraction period was significant at the 0.1 per cent level (t=7.00 for 29 df). A similar high level of significance was obtained for the differences between mean self-ratings (4.02 for the stress compared with 1.28 for the control group) for the Rorschach period (t=8.22).

As a group, the individuals in the control situation considered themselves as having slight anxiety or tension during subtraction with a tendency to be even more relaxed while taking the Rorschach. Individuals in the stress group reported themselves as more than moderately anxious or tense doing the subtraction and even more anxious (30 per cent of the group rated themselves at or near the extreme with ratings of 5 or 6) during the Rorschach. (A t of 1.85 for the difference between mean ratings during the subtraction and Rorschach periods for the stress group was significant between the 5 and 10 per cent levels of confidence.)

[8] Product-moment coefficients were computed when the range of scores was narrow with consequent numerous tied ranks.

Rorschach scores. All Rorschach protocols were coded and submitted for scoring to a skilled Rorschach interpreter who was not aware which protocols belonged to the stress group and which to the non-stress group. By having the coded but otherwise unidentified protocols scored independently in this way, any possible suggestive effect of knowledge of the *S* was eliminated.[9] The scoring followed Hertz's symbols (7) and norms (8). The protocols were then returned to the writer for psychogramming. In tabulating the results, each main response was counted as 1; each additional scoring counted as 0.5.

The means and standard deviations for the 15 Rorschach signs investigated are recorded in Table 2. Twelve of the 15 signs (*R, Rej, W, Do, Dr, M, Sh wt., C wt., Ad, Anat, P,* and *S.S.*) show means in the directions hypothesized. It is apparent from inspection of this table that the distributions of many of the Rorschach symbols are considerably skewed. Since the *t*-test becomes more valid and sensitive when the sampling distribution is normalized, the Rorschach symbols were transformed by taking the square root of the frequencies. Following the suggestion of Snedecor (21, p. 446), where the frequencies tended to be 10 or less for a particular symbol, the square root of the sum of the frequency plus 0.5 was used. A comparison of *t*'s for raw and transformed symbols is shown in Table 2. The general effect of the transformation was to raise the *t* obtained. Of the *t*'s for the 12 signs in the expected direction,

TABLE 2

MEANS AND SIGMAS OF RORSCHACH SCORES WITH *t*'s (RELATED MEASURES) FOR RAW AND TRANSFORMED SCORES

SCORE	GROUP	MEAN	S.D.	*t*	*P*(%)	*t*(transf.)	*P*(%)
R	Stress	22.95	14.61	2.10	5	2.32	2–5
	Non-stress	26.70	14.86				
Rej	Stress	.70	1.04	1.98	5–10	1.78	5–10
	Non-stress	.37	.66				
W	Stress	4.15	3.04	3.11	1	3.31	0.1–1
	Non-stress	6.63	3.79				
Do	Stress	.53	.67	1.06	30	1.84	5–10
	Non-stress	.33	1.11				
Dr	Stress	4.98	7.48	0.09	90	0.18	90
	Non-stress	4.90	6.34				
M	Stress	1.67	1.35	0.85	40	0.75	50
	Non-stress	1.98	1.64				
F	Stress	10.43	9.38	3.06	1	3.61	0.1–1
	Non-stress	14.20	10.22				
Sh wt.	Stress	3.52	2.95	2.45	2–5	2.58	1–2
	Non-stress	2.39	2.71				
C wt.	Stress	2.35	2.51	1.39	20	2.05	5
	Non-stress	2.97	1.73				
sum A	Stress	10.63	5.08	0.58	60	0.38	80
	Non-stress	11.33	6.82				
*Ad**	Stress	2.65	2.78	0.13	90	0.61	60
	Non-stress	2.57	3.90				
*Hd**	Stress	1.57	2.41	0.29	80	0.51	70
	Non-stress	1.67	2.07				
Anat	Stress	1.63	2.05	0.74	50	0.33	80
	Non-stress	1.33	1.22				
P	Stress	5.52	1.84	2.49	2	2.41	2–5
	Non-stress	6.62	2.28				
S.S.	Stress	4.00	1.77	0.81	50		
	Non-stress	3.63	1.68				

* To determine whether there was a significantly greater excess of human and animal details over human and animal forms for the stress group, a *t* for the relationship $(H+A)-(Hd+Ad)$ was also computed. The *t*'s were 0.50 for raw and 0.69 for transformed scores.

[9] Behavioral manifestations of stress and extra-test remarks were not recorded on the protocols to prevent any further clues as to which groups the protocols belonged to.

five (*W, Sh wt., P, R, C wt.*) are significant below the 5 per cent level while two (*Do, Rej*) are significant between the 5 and 10 per cent levels. Of the three signs (*F, Hd, sum A*) not in the hypothesized direction, the *t* for *F* is significant below the 1 per cent level.

An analysis of the 15 signs, using McNemar's formula (15, p. 207) for correlated proportions with a correction for continuity, was also made, each distribution being divided at its median. For oligophrenic details (*Do*) the proportion of individuals in the stress group giving one or more *Do* responses was significantly greater than the number in the control at the 5 per cent level of confidence (4.083 evaluated according to the chi-square distribution based on 1 *df*). The results for the remaining signs were, in general, lower than the significance values for the obtained *t*'s.[10]

Since the groups differ in the total number of responses given, it is possible that some of the differences obtained on the remaining signs may be partially or entirely reflecting this productivity difference. The usual method of controlling for this difference has been to convert the raw scores to percentages. As Cronbach (4) has indicated, this method introduces serious statistical problems and, in addition, fails, in most cases, to achieve the desired independence from total productivity.

To adjust for the differences in *R*, the method of covariance analysis was utilized. The method involves a determination of how much of the variation in the particular sign (*Y*) can be predicted from variation in productivity (*X*) and then subtracting this to secure the residual variance as the adjusted value. To test the significance of the difference between group means, the ratio of the adjusted treatment groups variance to the adjusted within groups variance was used.[11]

The method of covariance was applied only to those Rorschach signs which seemed to warrant its use. Two criteria were utilized in selecting the signs: a minimum significance level of 30 per cent and a linear relationship with total productivity (the latter being determined by inspection of scattergrams). On the basis of these criteria, five signs were selected: *W, F, Sh wt., C wt.*, and *P*. Each of these was then subjected to analysis of covariance. The independent variable used was the total number of responses, location, or determinant scores (whichever one was applicable) *minus* the total number of responses in the particular sign category under analysis (in the case of *Sh wt.* and *C wt.*, the number of shading or color responses was used). The purpose of sub-

TABLE 3

ANALYSIS OF COVARIANCE FOR TRANSFORMED RORSCHACH SCORES

SCORE	SOURCES	*df*	ADJUSTED VARIANCE	*F*	*P*(%)
Sh wt.	Treatment groups	1	3.77	12.52	0.1
	Within groups	57	0.30		
W	Treatment groups	1	4.56	8.33	1
	Within groups	57	0.55		
F	Treatment groups	1	5.46	4.56	5
	Within groups	57	1.20		
P	Treatment groups	1	0.46	3.30	7
	Within groups	57	0.14		
C wt.	Treatment groups	1	0.72	2.61	11
	Within groups	57	0.28		

[10] With only 30 pairs of individuals the use of correlated proportions is not an efficient technique for testing the significance of differences. The method also utilizes less of the available information than the *t*-test.

[11] A design involving the interaction variance (treatments by pairs) as error term was not profitable, since its utility depends upon the correlation of the interaction effects. The increase in precision effected by the type of covariance analysis employed in this study depends upon the within groups correlation between the two variables (12). This correlation (productivity vs. Rorschach symbol) was quite sizable for each of the scores subjected to such analysis. It should be noted that since matched groups were used, the within groups variance is, however, a somewhat gross measure of the error variance. As a consequence the results obtained probably are conservative.

tracting the number of responses in the dependent variable was to avoid any spurious correlations.

The results of the analyses using transformed scores are shown in Table 3. With the groups made comparable on the productivity factor, the obtained difference between the mean number of *Sh wt.* responses increased in significance to below the 0.1 per cent level. Differences on the remaining four signs decreased in significance. *W* declined to the 1 per cent level, *F* to the 5 per cent level, *P* to the 7 per cent level, and *C wt.* to the 11 per cent level.

Judges' ratings of anxiety. To obtain an over-all judgment of the extent of anxiety shown by the Rorschach protocols, the complete Rorschach record for each individual (with identification as to whether the individual was a member of the stress or non-stress group removed) was submitted to two skilled Rorschach interpreters for ratings on a four-point scale of anxiety. The following four degrees of anxiety were utilized: strongly anxious, 1; moderately but definitely anxious, 2; signs of anxiety but well within normal limits, 3; very little, if any, anxiety, 4.

The ratings for the two judges were combined, and the difference between mean ratings for the two groups (4.40 for the stress, 5.13 for the control group) was tested by the *t*-test for related measures. The obtained *t* of 2.45 just missed the 2 per cent level of confidence (for 29 *df* a *t* of 2.46 is required). Thus, the "global" picture derived from a study of the entire Rorschach record also revealed significant differences between groups in extent of anxiety.

DISCUSSION

Behavior under Stress

While anxiety has been defined primarily in terms of the experimental situation (presence versus absence of the stress condition), the significant differences in performance on the subtraction test and in self-ratings on the anxiety-tension scale afford important evidence of its presence in significant degree as a *behavioral* state in the *Ss* of the stress group.

Further evidence that the stress situation induced an appreciable degree of anxiety in the *Ss* is provided by their behavior and spontaneous comments in the experimental condition. Of the 34 *Ss* run in the stress situation, two, after signing the pledge and being strapped in the chair immediately prior to the Rorschach administration, asked to be excused. One complained of a bad headache; the other indicated that he had been in a "nervous" state before he came in because of certain conditions at home and felt he wasn't "up to it." Both were excused. (Since these two happened to be among the reserve *Ss* used in the experiment, no substitution of *Ss* was necessary.) Four others complained of headaches during the Rorschach inquiry, and their helmets were removed (in three cases, the headache disappeared on completion of the experiment). Two others, during the inquiry, showed concern that their hearts were beating very rapidly. In these cases, the motor was stopped and the belts removed. Most of the remaining *Ss* were in some state of visible agitation, the specific signs of which were frequent deep and rapid breathing, sighing, grunting (on receipt of shock), sweating, and trembling. Several sat with fists clenched or gripped the arm of the chair, bracing themselves. Some of the comments made at the time of the self-ratings are also revealing: "I was very tense—scared to death of electricity all my life. I gripped the chair with my left hand. I was afraid to take my hand off the chair." "I felt extremely nervous at being strapped in, particularly because of the one across my forehead. I just couldn't concentrate on what I was doing. I tried to but the feeling of anxiety was a stronger influence." "I tried hard to see—I was waiting for the shock and still tense. I was nervously aroused—the blot just didn't seem to coordinate any object I knew of. I was more tense since I was expecting to get shock on both sides of my cheeks and hands and also on the head." "I was so anxious that I just couldn't see anything. It just looked like a blank piece of paper." "I really expected shock and when you told me, if I didn't get it for a while, it would be a lot worse, then I really expected it." "I didn't mind the three but if it was going to be worse than that and both sides—then!!! I couldn't get any more nervous than that." A senior in electrical engineering said: "After strapping in I

figured I'd have to be set for a pretty big shock. I know I wasn't perfectly relaxed looking at those cards. I must have been pretty tense." A junior in physics indicated at the time the helmet was put on: "This should be an experience. I never was shocked in the head before." At the time of the self-rating, the same *S* indicated: "The pledge made me think of the time in high school when I had to sign a pledge which didn't let anyone take a course in Electrical Engineering if they had a weak heart, and that made me tense. I sort of relaxed on the last cards—I figured there probably would be no more shock. But then as I started to relax, I figured maybe they want me to relax so that the next one would come as a surprise and really be something." A few *Ss* reported a "tingling" or mild shock during the Rorschach.

The Rorschach Signs of Anxiety

Of the 15 alleged Rorschach anxiety signs investigated, four of them (*Sh wt., W, R, Do* [12]) reflected the differential experimental conditions in the manner originally hypothesized at a high or moderate level of statistical reliability.[13] Three other signs (*P, Rej, C wt.*) did not reach an acceptable statistical level of significance but came sufficiently close to warrant considering them as suggestive findings. How do these results relate to the clinical concepts of anxiety?

One of the major effects of anxiety has been described by clinicians as a "constriction," "hemming-in," or "inhibition" of personality, entailing a reduction in the flexibility of behavior. If we examine the performance of the stress group on the Rorschach test, we find that the changes in the perceptual and associative processes in large part support this notion of the constrictive effect of anxiety. Thus the stress-produced anxiety (evaluating the Rorschach signs in the usual interpretative terms) reflected itself in an impairment of ability to survey, abstract, or generalize (decreased number of whole responses). It adversely affected the general pliancy and wealth of the perceptual and associative processes (decreased number of responses and increased number of card rejections). It arrested possible integrative tendencies (increased oligophrenic details). Thinking along conventional lines ("common sense") was impaired (decreased number of popular responses). The increased weighted number of shading responses and decreased weighted number of color responses also attest to the blocked perceptual processes.[14]

Somewhat similar findings of the detrimental effects of stress on perception have been reported by Postman and Bruner (17). Two groups of *Ss* were required to recognize three-word sentences presented in a tachistoscope, one group being subjected to a stress situation involving perceptual frustration and disruptive criticism from the *Es*. These investigators found that the stress situation adversely affected the "ability to select percepts from a complex stimulus field" (p. 321). They also concluded that stress "affected not only the ability to perceive a complex stimulus in its entirety but the capacity to structure less complex components as well" (p. 318). (Perception of whole sentences, as well as the process of building up sentences word by word, was impaired.)

As previously indicated, one of the alleged anxiety signs (number of responses determined solely by form) showed a difference at a high level of significance in the direction opposite to that hypothesized.[15] Form responses are regarded as an index of conscious effort and control. A high number of *F* responses is considered as evidence of narrowed (over-controlled) emotional life. The explanation for the reversal in the *F* sign appears not to be that of an increase of affective and phantasy life in the stress group (the

[12] As indicated above the difference in *Do*'s was found to be significant by analysis of correlated proportions.

[13] It will be recalled that the groups were not originally matched for *W* on the Behn. Since whole responses were scored on this test, however, it was possible to compare the two groups for number of *W*. The mean number of "wholes" for the stress group was 8.70, for the control 10.28. This difference is not statistically significant. A check was made to determine whether this difference could account for the significant difference obtained for the Rorschach *W* score. Twenty of the matched pairs of individuals were selected so that group means and standard deviations for the Behn *W* score were very closely matched. The difference for the Rorschach *W* score (transformed and equated for productivity by analysis of covariance) was significant at close to the .03 level of confidence.

[14] Cf. Douglas (5) and Rapaport (18).

[15] On the Behn, the mean number of *F* responses for the stress group was 12.37, for the non-stress group 11.60. This difference is not statistically significant.

mean number of *M* and *C wt.* responses was lower than in the control group), but, rather, it appears that the individuals in the stress group gave proportionately more responses involving *both* form *and* shading as determinants. If the percentage of so-called "undifferentiated" *F* responses (total number of responses to form excluding human movement and color)[16] is used as an index of intellectual control, it is found that the differences are in the hypothesized direction. Thus, the stress group has an undifferentiated *F*% of 78.8, whereas the control group's undifferentiated *F*% was 74.4. This suggests that an adequate appraisal of formal control should probably involve *both* the *F* refined *and F* undifferentiated indices.[17]

The positive results obtained in the case of four (*Sh wt., W, R, Do*) of the alleged Rorschach anxiety signs provide experimental evidence of their significance as anxiety indicators, while the suggestive findings in favor of three additional signs (*P, Rej, C wt.*) lend some support for their utility. The failure to obtain significant results in the direction hypothesized for the remaining eight signs is open to several interpretations. It may be that they possess no utility as anxiety indicators and that the assignment of these factors to this category on the basis of the allegations of Rorschach experts was actually in the nature of unfortunate guesses. Another possibility is that these signs are not sensitive to the particular type of stress-produced anxiety involved in the present experiment but rather, are indicators of a more "ego-involved" anxiety or of a type of anxiety related to psychopathologic personality development. Still another possibility is that the results are a function of certain characteristics of the group of *S*s investigated, e.g., higher-than-average intelligence, comparative youth. Nevertheless, in the absence of studies which would serve to implicate one or another of the possibilities mentioned above, the data must be accepted for what they do indicate, viz., that a number of Rorschach factors which have been alleged, on the basis of inadequate evidence, to be "anxiety indicators" fail to meet this particular test of experimental validation.

One implication of the results with respect to Rorschach interpretation should be mentioned. In line with the findings of such investigators as Kimble (10) and Lord (13), the results of this experiment indicate that situational factors do influence test performance. These findings conflict with the view held by some experts that the Rorschach test taps some self-contained entity ("basic personality structure") unrelated to any external variables.[18] It would appear, however, that such situational factors, far from being irrelevant, are important determiners of test responses and should be carefully considered in any evaluation of Rorschach results.

[16] This is Rorschach's original "*F*."

[17] Beck (2, p. 49) indicates that the total number of *non-F* responses is low "in the anxious of average or lower endowment." The failure to attain an increase of *F* (refined) responses in the stress group may thus possibly be related to the better-than-average intelligence of the sample of *S*'s employed in the present study.

SUMMARY

1. The purpose of the present study was to investigate the influence of a stress-produced anxiety on the occurrence of 15 Rorschach factors alleged to be signs of anxiety.

2. These 15 factors and the hypothesized directions of change in the stress situation were as follows:

a) Decreased number of responses (*R*).
b) Increased number of card rejections (*Rej*).
c) Decreased number of whole responses (*W*).
d) Increased number of oligophrenic details (*Do*).
e) Increased number of rare details (*Dr*).
f) Decreased number of human movement responses (*M*).
g) Increased number of weighted shading responses (*Sh wt.*).
h) Increased number of responses determined solely by form (*F*).
i) Decreased number of weighted color responses (*C wt.*).
j) Increased number of animal responses (*sum A*).
k) Increased number of animal details (*Ad*).
l) Increased number of human details (*Hd*).

[18] Cf. Piotrowski's view (16).

m) Increased number of anatomy responses (*Anat*).

n) Decreased number of popular responses (*P*).

o) Increased number of shading shock signs (*S.S.*).

3. Sixty college students were matched on certain factors on the Behn-Rorschach test as well as on age, college year, and cumulative grade point average. One group of 30 students was then assigned to the experimental (stress) condition, the other group to the control (non-stress) condition. "Stress" consisted of the administration of electric shock of increasing intensity, the implication by the *E* of forthcoming applications of stronger shock, and the use of certain equipment for suggestive effects. Immediately prior to the administration of the Rorschach test, both groups were given a continuous subtraction test. Individuals in the stress condition received shock while performing on the subtraction task. They received no shock while taking the Rorschach. Following the Rorshach individuals in both groups rated themselves on an anxiety-tension scale.

4. The results obtained were as follows:

a) Subjects in the stress group made significantly more errors on the subtraction test than did the control group.

b) On self-ratings of subjectively experienced anxiety or tension the *Ss* in the stress group were found to be significantly more anxious or tense than the *Ss* in the control group.

c) Of the 15 alleged indices of anxiety investigated, four (*Sh wt., W, R, Do*) reflected the differential conditions in the manner hypothesized at a high or moderate level of statistical reliability. Three other signs (*P, Rej, C wt.*) did not reach an acceptable level of statistical significance but came sufficiently close to warrant considering them as suggestive findings.

d) One indicator (responses determined by form alone, i.e., "refined" *F*) showed a difference at a high level of significance in the direction opposite to that hypothesized. Analysis of the data indicates that the increase in shading responses was the major determinant of the observed difference in the number of "refined" *F* responses. When Rorschach's original scoring for *F* ("undifferentiated" *F*) was used, the difference in percentage of such responses was in the hypothesized direction.

5. The positive results were considered as affording experimental evidence of the importance of *Sh wt., W, R, Do* categories as anxiety indicators. Near-significant findings for the *P, Rej, C wt.* signs were considered as furnishing suggestive evidence of their utility in this regard.

6. The failure to obtain significant results in the directions hypothesized for the remaining signs was considered as experimental evidence of their insensitivity to the stress-produced anxiety of the type involved in this experiment. Possible interpretations of the significance of these negative findings were discussed.

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TESTING FOR THE EXISTENCE OF PSYCHOMETRIC PATTERNS [1]

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An analysis of variance technique applicable as an over-all test for the existence of group psychometric patterns will be described. This procedure is offered to the clinical psychologist who is interested in establishing configurations of test scores as aids in psychiatric diagnosis or personality evaluation. The presentation, and use, of the procedure presupposes some knowledge of the analysis of variance.

Given V variables (tests or subtests) and G groups with m individuals per group. Let X_{giv} stand for the score on variable v of individual i in group g, and let n=total number of individuals and N=total number of scores ($n=mG$, and $N=mVG$). The scores can be arranged as for a three-way analysis of variance setup, with the V variables corresponding to columns, the G groups corresponding to, say, blocks, and the m rows within each group (or block) corresponding to individuals. The schema is outlined in Table 1, which also includes an indication of pertinent sums and means. It will, of course, be noted that this setup differs from the ordinary three-way classification layout in that the scores in any one block (group) are independent of the scores in any other block—a given individual has scores in only one block. With this clearly understood, we may safely use the usual notation. Since the ith individual in one block does not correspond to the ith individual in the other blocks, we see immediately that summing across or through blocks is meaningless, and accordingly there can be no ordinary row by column interaction.

It will be noted that summing scores across columns leads to a total score, and a mean, for each individual. Summing down within a group leads to separate group means for the several variables, and summing these sums across blocks leads to the means at the bottom for each variable for the groups combined. The separate group means for the several variables are, of course, the basis for possible group profiles or group psychometric patterns. Before deciding that the profile for one group differs from that for another, or from the pattern of the means for the groups combined, we need a test of statistical significance. Those versed in the analysis of variance will by now have realized that the question as to whether the profiles differ significantly is nothing more than the question as to whether the group by variable (block by column) interaction is significant. This can be answered as soon as we have determined the proper error term for the variance ratio.

The breakdown of the sum of squares and degrees of freedom, according to sources of variation, is given in Table 2. It will be seen that we have three main sources of variation, but that one of them, the individual difference component, does not correspond to the usual row variation. Actually, this component is exactly analogous to an ordinary within groups term; here it is weighted by V. We have already indicated that there can be no meaningful row by column interaction in the ordinary sense of the three-way classification. At first thought, this might seem to imply that for the present problem there is no possibility of an individual by variable interaction. However, when we scrutinize the expression for the residual sum of squares, we see that we have a term which is much like that of the ordinary row by column interaction. It involves an individual by

[1] Published with permission of the Chief Medical Director, Department of Medicine and Surgery, Veterans Administration, who assumes no responsibility for the opinions expressed or the conclusions drawn by the authors.

TABLE 1

SCORE SCHEMA FOR V VARIABLES, WITH n CASES FOR EACH OF G GROUPS

BLOCKS (groups)	ROWS (cases)	COLUMNS (variables) 1	v	V	SUM	MEAN
	1	X_{111}	X_{11v}	X_{11V}	$\sum_v X_{11v}$	$\bar{X}_{11.}$
	i	X_{1i1}	X_{1iv}	X_{1iV}	$\sum_v X_{1iv}$	$\bar{X}_{1i.}$
(1)	n	X_{1n1}	X_{1nv}	X_{1nV}	$\sum_v X_{1nv}$	$\bar{X}_{1n.}$
	Sum	$\sum_i X_{1i1}$	$\sum_i X_{1iv}$	$\sum_i X_{1iV}$	$\sum_i \sum_v X_{1iv}$	
	Mean	$\bar{X}_{1.1}$	$\bar{X}_{1.v}$	$\bar{X}_{1.V}$	$\bar{X}_{1..}$	
	1	X_{g11}	X_{g1v}	X_{g1V}	$\sum_v X_{g1v}$	$\bar{X}_{g1.}$
	i	X_{gi1}	X_{giv}	X_{giV}	$\sum_v X_{giv}$	$\bar{X}_{gi.}$
(g)	n	X_{gn1}	X_{gnv}	X_{gnV}	$\sum_v X_{gnv}$	$\bar{X}_{gn.}$
	Sum	$\sum_i X_{gi1}$	$\sum_i X_{giv}$	$\sum_i X_{giV}$	$\sum_i \sum_v X_{giv}$	
	Mean	$\bar{X}_{g.1}$	$\bar{X}_{g.v}$	$\bar{X}_{g.V}$	$\bar{X}_{g..}$	
	1	X_{G11}	X_{G1v}	X_{G1V}	$\sum_v X_{G1v}$	$\bar{X}_{G1.}$
	i	X_{Gi1}	X_{Giv}	X_{GiV}	$\sum_v X_{Giv}$	$\bar{X}_{Gi.}$
(G)	n	X_{Gn1}	X_{Gnv}	X_{GnV}	$\sum_v X_{Gnv}$	$\bar{X}_{Gn.}$
	Sum	$\sum_i X_{Gi1}$	$\sum_i X_{Giv}$	$\sum_i X_{GiV}$	$\sum_i \sum_v X_{Giv}$	
	Mean	$\bar{X}_{G.1}$	$\bar{X}_{G.v}$	$\bar{X}_{G.V}$	$\bar{X}_{G..}$	
	Σ sums	$\sum_g \sum_i X_{gi1}$	$\sum_g \sum_i X_{giv}$	$\sum_g \sum_i X_{giV}$	$\sum_g \sum_i \sum_v X_{giv}$	
	Mean	$\bar{X}_{..1}$	$\bar{X}_{..v}$	$\bar{X}_{..V}$	$\bar{X}_{...}$	$= \bar{X}$

TABLE 2

VARIANCE TABLE

SOURCE	SUM SQS.	df	VAR. EST.
Variables	$n\sum_v (\bar{X}_{..v}-\bar{X})^2$	$V-1$	s_v^2
Groups	$nV\sum_g (\bar{X}_{g..}-\bar{X})^2$	$G-1$	s_g^2
Individuals	$V\sum_{gi} \sum (\bar{X}_{gi.}-\bar{X}_{g..})^2$	$n-G$	s_i^2
V by G interaction	$n\sum_{gv} (\bar{X}_{g.v}-\bar{X}_{g..}-\bar{X}_{..v}+\bar{X})^2$	$(G-1)(V-1)$	s_{gv}^2
Residual	$\sum_{giv} \sum (X_{giv}-\bar{X}_{gi.}-\bar{X}_{g.v}+\bar{X}_{g..})^2$	$N+G-n-VG$	s_r^2
Total	$\sum_{giv} \sum (X_{giv}-\bar{X})^2$	$N-1$	

variable interaction obtained separately for each group, then summed over groups. That this residual term is an individual by variable interaction receives further verification from the fact that its *df* can be written as the product of the *df*'s of the variable and the individual difference components. Since the five components indicated in Table 2 exhaust all the degrees of freedom, it follows that there can be no individual by group interaction.

The residual becomes the basis for an appropriate error variance for testing the significance of the V by G interaction. That is, we take $F=s^2_{gv}/s^2_r$, with the *df*'s as specified in Table 2. If this F is significant, we can conclude that the configurational differences in group profiles are non-chance or real. No qualifications need be made if the several profiles happen to fall at different levels.

Parenthetically, it might be pointed out that while s^2_r is the proper error term for testing the differences due to variables, it is not the correct term for testing the group differences. For this purpose we would need to use s^2_i as the denominator in the variance ratio.

The required computations for the total, the group (block), the variable (column), and the block by column interaction sums of squares are precisely the same as in a three-way analysis of variance (4, pp. 299–305), whereas for individuals we have the ordinary within groups sum of squares, calculable via the usual formula (4, p. 242) with the factor $1/m$ replaced by $1/mV$. In terms of the sums along the right-hand side of Table 1, the individual difference sum of squares is given by

$$\frac{1}{mV}\left[m\sum_g\sum_i(\sum_v X_{giv})^2-\sum_g(\sum_i\sum_v X_{giv})^2\right].$$

The residual sum of squares is obtained by subtraction: total minus the four components.

The foregoing is general. Before considering specific applications, the underlying assumptions should be mentioned: normal score distributions with equal variances for each group on each variable. The departures from these assumptions should not be greater than one would expect on the basis of sampling errors. Thus, only variables (tests or subtests) with normalized standard scores are permissible. This is the usual requirement for profile purposes.

A number of psychological instruments, such as the Wechsler-Bellevue, the MMPI, the Kuder Preference Record, the Strong Vocational Interest Blank, the Psychological Corporation's Differential Battery, also certain standard achievement batteries, utilize standard scores for each subtest and are therefore amenable to the analysis of variance test for group patterns. Although Rorschach score distributions are usually markedly skewed, a number of relevant determinants may readily be adapted for use with the foregoing technique by converting the scores into standard normal form by T-scaling. Rorschach scores deemed partially equivalent may be combined in order to spread out the distribution.

Of special interest is the use of the technique as an objective method for determining whether patterns exist for various diagnostic groups.[2] Applications of this method should resolve the conflicting opinion resulting from claims set forth on the basis of clinical impressions and minimal quantitification (6, pp. 150–151). In a sense, the existence of group patterns might be regarded as prerequisite to any utility of the pattern for an individual. There is, of course, a different approach such as the pattern method advanced by Cronbach (2), which starts with individual patterns but is unfortunately limited to only three variables.

Any applications of the technique, and subsequent use of demonstrated patterns, must take into account a number of sources of error. Ambiguous nosology and the unreliability of psychiatric diagnosis (1) are attenuating factors, as are also the low reliabilities of certain subtest scores (3). There is also the problem of representative sampling of diagnostic groups (and normals) and size adequacy of samples. Although the variance method sketched here is applicable for either small or large samples, it is doubtful whether

[2] W-B, MMPI, and Rorschach patterns are currently being evaluated by this analysis of variance technique. The results will be described under the following titles:

L. S. Levine. "An investigation of Psychometric Patterns in Psychiatric Diagnoses."

J. Block. "A Pattern Analysis of the Minnesota Multiphasic Personality Inventory."

W. A. Cass, Jr. "Quantitative Rorschach Patterns as Related to Clinical Diagnoses."

the null hypothesis regarding patterns is apt to be rejected when group sizes are as low as in some studies (5). It should not be necessary to remark that the categorization of individuals into groups should not be based on prior knowledge of possible patterns.

In summary, an analysis of variance technique as a test for the existence of group psychometric patterns has been described. The method depends upon the fact that profile differences from group to group reflect themselves in an interaction term. Possible applications and limitations of the technique have been briefly discussed.

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A PERSONALITY SCALE FOR DOMINANCE[1]

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THE present study is part of a project concerned with political behavior and, especially, political participation.[2] The purpose of the larger project is to investigate the validity of certain ideas of political scientists, political leaders, and citizens about the nature and operation of the democratic process, and to discover how realistically they take into account the facts of political life in our country. These ideas usually presuppose a developed sense of *participation, awareness,* and *responsibility* among both the leaders and the governed of the American body politic. In the light of such "mass society" developments as urbanization, the enormous increases in population, the increasing complexity of social living, the growing emphasis on specialization, and the vast extension of governmental functions, it becomes of increasing importance to determine the character of political participation, awareness, and responsibility, how they develop, how they may be influenced, and how they manifest themselves.

The general plan of the project will be described in other publications, but it is pertinent to outline briefly the research setting for the present study, and the manner in which it fits into the program. The basic research problem is to discover and specify the various factors—sociological, psychological, and so forth—which underlie and relate to political participation. The variable of participation is treated as a criterion, and the task is to ascertain what other factors are related to it. Participation is assumed to be a function of factors x_1, x_2, x_3,...x_n, and the goal of the research is the identification and description of these factors. We are, of course, concerned with "prediction" mainly in the statistical sense, since our chief aim is the exploration of causal factors of participation.

Preliminary theoretical analysis suggested that factors such as "awareness," sense of responsibility, socio-economic status, sense of community identification, feelings of bewilderment and confusion, intellectuality, dominance, and so forth might play a part, and attention was accordingly directed to techniques for assessing these variables.

In translating this kind of thinking into an actual research program, the first task is to obtain a clearly defined and tangible criterion and then to find reliable methods for evaluating the predictor variables and for relating them to the criterion.

We considered dominance to be a particularly important dimension in relation to political participation, and the decision was therefore made to construct a new personality scale which would embody elements not present in extant devices, and which would, furthermore, correlate closely with group ratings on dominance.

DEVELOPMENT OF THE SCALE

The technique utilized in constructing the dominance scale was one which might be called the "peer group nomination technique," as distinguished from a strictly sociometric procedure. Subjects were not asked to choose the group member they would most or least prefer to see in some specified role, but were asked to nominate the members of their group whom they considered to be most and least dominant. A number of different groups cooperated in carrying out this phase of the study, including one fraternity and one sorority at the University of

[1] This is one of a series of studies on the broad problem of social responsibilities conducted in the Laboratory for Research in Social Relations, University of Minnesota, under a grant from the Carnegie Corporation.

[2] Dr. Kenneth E. Clark, Department of Psychology, University of Minnesota, is a member of the research team for the larger project, and has given valuable help in the completion of the present study as well.

Minnesota,[3] and several classes in social science at North High School in Minneapolis.[4] Approximately one hundred students were included in the college sample, and 124 were in the high school sample.

Each student participating in the study was given a personal data sheet calling for routine information, for dominance selections, and for dominance self-ratings. Instructions were included defining the dimension of dominance. It was an interesting observation that none of the students seemed to feel a need for this specification, and many stated that they could have completed the choices and ratings equally well without any guide other than the trait name itself. This fact is mentioned in support of the contention that the concept "dominance," for all its complexities, is a functioning term in the vocabularies of unsophisticated persons, a term which they apply in their everyday evaluations.

That these judgments, furthermore, are not merely fortuitous or idiosyncratic is established by the concordance in ratings to be reported shortly, and further documented by the repetitive validity of a number of subtle personality test items in the two groups. In short, the concept "dominance" appears to have both empirical content and social relevance. Whether or not it has significance in regard to the broader questions of political participation remains to be seen.

The directions for the nomination section are reproduced below:

> The dominant person tends to be the "stronger" in face-to-face personal situations, for instance, with friends, salespeople, etc. He is able to influence others, to gain their automatic respect, and if necessary to control them. He is not readily intimidated or defeated, and his own feelings in most face-to-face situations seem to be feelings of safety, security, personal rightness, and self-confidence. Such people are often described by others as "forceful", "masterful", "strong", "confident", "authoritative", and "sure of himself".
>
> People with *low* dominance are submissive. They appear and feel weaker in face-to-face contacts. They find it hard to assert themselves, to stand up for their rights and opinions, and are more easily influenced and intimidated by others. When a dominant and submissive person come into conflict, the dominant one gets his way.
>
> There are three things to be especially careful about in rating dominance:
>
> 1. Do *not* assume that "dominance" means domineering or "autocratic". While most domineering people are dominant, not all dominant people are domineering. Dominant people do not necessarily become leaders, or go out of their way to control and influence others. But whether they do or not, they create this effect of masterful self-assurance and others are often influenced by them. They may be very fair and reasonable: they may not want to lead people; yet still this basic dominance shows through.
> 2. Please rate on the basis of the person's *actual behavior*, not on the basis of how you think he would like to act, or how he fancies himself in his daydreams, etc. We want to know how dominant he is in actual behavior.
> 3. Remember that some people get into positions of prestige and power and thus control others, even though they may not be particularly dominant, for instance, because they are very bright, or have much money, etc. Do not confuse this with the *personal trait* of dominance, which we are asking you to judge.

Following this set of instructions, the high school students were asked to list the names of the five most dominant, and five least dominant in that particular classroom. (All ratings were taken in social science classes so that no student voted, or was voted upon, more than once.) In the college samples, students were asked to list the ten most and ten least dominant members of the fraternity or sorority to which they belonged. The self-rating section utilized the graphic-rating technique with a four-inch scale calibrated in half-inch intervals.

In order to select the criterion groups for the item analyses, weights of plus 1 for each "most dominant" choice, and of minus 1 for each "least dominant" choice, were assigned. Adding these weights gave a "score" to each subject, on the basis of which rankings and selections were made. There was a broad range of scores in each group, indicating that certain students were clearly seen as dominant and others just as distinctly seen as submissive. The self-ratings did not show the same spread of scores, for, as might be expected, not very many subjects were willing to rate themselves as "submissive." There was a noticeable piling up of scores, nevertheless, at the middle, at the most dominant extreme, and the least dominant extreme, in that order. The correlation between the

[3] The writers wish to thank Dean E. G. Williamson of the University of Minnesota for his kindness in facilitating this aspect of the research.

[4] These *Ss* were obtained through the courtesy of Dr. Kopple Friedman of the Minneapolis Public Schools.

group- and self-ratings in the college sample was .421, *SE* .14, and in the high school sample .441, *SE* .09.

Prior to obtaining the nominations in each group, we administered a questionnaire consisting of some 150 items which were especially written to assess the dominance factor, as it was then conceptualized by the writers. In the college group, subsamples composed of the 25 most and 25 least dominant students were selected (13 females, 12 males in each subsample) for the item analysis. In this total group there was no question of whether males or females were most dominant, as the rankings were made separately for the fraternity and for the sorority, and the extreme cases were pooled for the subsequent analysis. In the high school group, subsamples of 25 at each extreme were chosen (12 females, 13 males in each). The ratings here did indicate a slight tendency toward greater dominance for males.

A number of college students had taken the Minnesota Multiphasic Personality Inventory (2) as part of a routine testing procedure at other times, and on the basis of those records which were available to us, the responses of 16 more and 16 less dominant college students (8 females, 8 males) were studied. Approximately 100 items from the MMPI showed promise in discriminating these two subsamples, and these items were, accordingly, administered to the high school classes a few days after the previously mentioned questionnaire. Each of the criterion subgroups for this second analysis included 12 females and 12 males.

From the two item analyses in each of the two groups (high school and college) a total of 60 items was retained, all of which revealed differentiating power in both samples. Thirty-two of these items were drawn from the specially written pool, and 28 from the MMPI. Table 1 lists these items.[5]

Inspection of these items reveals some interesting trends. It would appear that no more than half are immediately obvious, in the sense that they deal explicitly with matters of superordination, control, strength, and ascendancy. A few of the items are quite subtle, in the sense that they would not ordinarily be thought of as correlates of dominance (e.g., "Sometimes at elections I vote for men about whom I know very little," answered "Agree"), and one or two actually seem to work "backwards," (e.g., "I sometimes act like a coward," answered "Agree").

A careful reading of the items suggests a number of characterizations of the subjective side of the dominant personality. The factor which is implied by the largest number of items appears to be one of poise and self-assurance. The dominant personality maintains a high level of self-confidence, does not seem to be plagued by self-doubts or equivocations, and therefore appears freer to behave in an unencumbered and straightforward manner. The impression given is one of resoluteness and vigorous optimism. Closely related to this is another suggested factor of resourcefulness and efficiency. The dominant personality appears to move forward in a realistic, task-oriented fashion and manifests feelings of adequacy in meeting whatever obstacles may be encountered.

There is also a certain element of perseverance, or even doggedness, implied. The dominant subjects admit to working on at things even when others become impatient with them, etc., and in general give evidence of strong completion-needs.

The dominant *S*s indicate what might be called a "dutiful" sense of morality. They agree that people should pay school taxes even if they have no children, that everyone must get behind a community decision regardless of previous sentiments on the issue, that we should curtail our own use of oil in order to preserve a fair share for future generations, and state that they would be willing to give money to right a wrong which was no particular concern of theirs. Dominant persons, as so visualized, would be expected to take active roles in getting group members to accept the collective responsibilities of the group. They would be the ones who would insist on distributed sacrifices and obligations in carrying out group projects.

[5] A more complete table giving the percentages in each group responding with "True," and the significance tests, is on file with the American Documentation Institute. Order Document 2932 from American Documentation Institute, 1719 N Street, N.W., Washington 6, D. C., remitting $0.50 for microfilm (images 1 in. high on standard 35 mm. motion picture film) or $0.50 for photocopies (6 in. x 8 in.) readable without optic aid.

Trends such as these might also suggest an element of rigidity and inflexibility, but it appears on closer investigation to be not so much a matter of rigidity in the ordinary sense of routinization of method and procedure, as an unyieldingness in the area of goals and values. The dominant personality would show adaptiveness in relation to means and techniques, but less in respect to aims and purposes.

The high scorers would also seem to be characterized by a rather deep-seated seriousness. This does not imply solemnity nor pomposity, but would appear to reflect a basic

TABLE 1

Items Included in the Dominance Scale, and Responses Scored for "Dominance"

1. A person does not need to worry about other people if only he looks after himself. (Disagree)
2. When I work on a committee I like to take charge of things. (Agree)
3. I enjoy planning things, and deciding what each person should do. (Agree)
4. I'm not the type to be a political leader. (Disagree)
5. People seem naturally to turn to me when decisions have to be made. (Agree)
6. I would be a good leader of people. (Agree)
7. I have a natural talent for influencing people (Agree)
8. I like to give orders and get things moving. (Agree)
9. I hate to have to tell others what to do. (Disagree)
10. I think I would enjoy having authority over other people. (Agree)
11. I would rather not have very much responsibility for other people. (Disagree)
12. I doubt whether I would make a good leader. (Disagree)
13. Disobedience to any government is never justified. (Disagree)
14. I think I am usually a leader in my group. (Agree)
15. Every citizen should take the time to find out about national affairs, even if it means giving up some personal pleasure. (Agree)
16. There are times when I act like a coward. (Agree)
17. People should not have to pay taxes for the schools if they do not have children. (Disagree)
18. I am embarrassed with people I do not know well. (Disagree)
19. When the community makes a decision, it is up to a person to help carry it out, even if he had been against it. (Agree)
20. I would be willing to give money myself in order to right a wrong, even though I was not mixed up in it in the first place. (Agree)
21. School teachers complain a lot about their pay, but it seems to me they get as much as they deserve. (Disagree)
22. In a group, I usually take the responsibility for getting people introduced. (Agree)
23. I am a better talker than listener. (Agree)
24. I would rather have people dislike me than have them look down on me. (Agree)
25. I would be willing to describe myself as a pretty "strong" personality. (Agree)
26. We should cut down on our use of oil, if necessary, so that there will be plenty left for the people fifty or a hundred years from now. (Agree)
27. I dislike to have to talk in front of a group of people. (Disagree)
28. I must admit I am a pretty fair talker. (Agree)
29. We ought to let Europe get out of its own mess; it made its bed, let it lie in it. (Disagree)
30. I must admit I try to see what others think before I take a stand. (Disagree)
31. When prices are high you can't blame a person for getting all he can while the getting is good. (Disagree)
32. I can honestiy say that I do not really mind paying my taxes because I feel that's one of the things I can do for what I get from the community. (Agree)
33. I find it hard to keep my mind on a task or job. (Disagree)
34. I have not lived the right kind of life. (Disagree)
35. I sometimes keep on at a thing until others lose patience with me. (Agree)
36. I am easily downed in an argument. (Disagree)
37. I am certainly lacking in self-confidence. (Disagree)
38. I do many things which I regret afterwards (I regret things more or more often than others seem to). (Disagree)
39. I frequently notice my hand shakes when I try to do something. (Disagree)
40. I very much like hunting. (Disagree)
41. My parents have often objected to the kind of people I went around with. (Disagree)
42. I should like to belong to several clubs or lodges. (Agree)
43. I never worry about my looks. (Disagree)
44. I believe there is a Devil and a Hell in afterlife. (Disagree)
45. I don't blame anyone for trying to grab everything he can get in this world. (Disagree)
46. Sometimes at elections I vote for men about whom I know very little. (Agree)
47. When in a group of people I have trouble thinking of the right things to talk about. (Disagree)
48. Something exciting will almost always pull me out of it when I am feeling low. (Disagree)
49. When I leave home I do not worry about whether the door is locked and the windows closed. (Agree)
50. In school I found it very hard to talk before the class. (Disagree)
51. I usually have to stop and think before I act even in trifling matters. (Disagree)
52. I have more trouble concentrating than others seem to have. (Disagree)
53. I have sometimes stayed away from another person because I feared doing or saying something that I might regret afterwards. (Agree)
54. The future is too uncertain for a person to make serious plans. (Disagree)
55. I played hooky from school quite often as a youngster. (Disagree)
56. I have strong political opinions. (Agree)
57. Christ performed miracles such as changing water into wine. (Disagree)
58. I practically never blush. (Agree)
59. A large number of people are guilty of bad sexual conduct. (Disagree)
60. The one to whom I was most attached and whom I most admired as a child was a woman (mother, sister, aunt, or other woman). (Disagree)

acceptance of self and a consequent disinclination to treat fundamental needs, satisfactions, and dissatisfactions in a light or trivial manner. Thus, the dominant *Ss* deny that something exciting will almost always pull them out of a low mood, agree that they have strong political opinions, and deny that they never worry about their physical appearance. There is almost a compulsive need to face reality. (For example, the high scorers admit that they sometimes vote for unknown candidates at an election, and that they sometimes act in a cowardly manner, even though the latter description would probably not be applied to their behavior by friends and acquaintances.) Similar tendencies might account for the denial of certain conventionally accepted religious beliefs by the dominant subjects.

It should be emphasized at this point that the authors are merely elaborating some of the impressions and inferences which they have obtained from a consideration of the items listed above. These speculations are not offered as conclusions, or as demonstrated findings, but as hypotheses for further investigation.

The next consideration in the development of the dominance scale was the question of reliability. The only evidence so far observed on this point comes from a coefficient based on the Kuder-Richardson formula No. 21, of .79 for the combined distribution of the two original samples.

On the more important question of validity, the evidence cited in Table 2 may be offered. The number of cases in the two subsamples for each group is rather small, an unavoidable limitation because not all of the *Ss* in both groups were exposed to all of the items finally selected to make up the 60-item scale. It can be seen, nevertheless, that for the cases scored on the total scale, the discrimination between most and least dominant subsamples was very good. With the exception of two cases in the college samples, and four in the high school samples, the two distributions are non-overlapping. The percentage of correct sortings which would be achieved with a cutting score of 36 in the college sample would be 94, and a cutting score of 39 in the high school sample would correctly classify 92 per cent of the cases. Validation in samples beyond the two already reported has not as yet been carried out.

The correlation between scores on the dominance scale and the dominance ratings was .599, *SE* .14, in the college sample, and .687, *SE* .09, in the high school group. The coefficient between self-ratings and the personality scale was .520, *SE* .14, in the college group, and .555, *SE* .09, for the high school.

TABLE 2

DISTRIBUTION OF RAW SCORES ON THE DOMINANCE SCALE FOR EACH OF THE TWO SUBSAMPLES ("MOST" AND "LEAST" DOMINANT) IN THE TWO CRITERION GROUPS

	Criterion Groups			
	College		High School	
	M	L	M	L
53–54	2			
51–52	2		1	
49–50	1		2	
47–48	3		5	
45–46	2		2	
43–44	3		1	
41–42	1		6	
39–40	0	2	3	
37–38	0	2	2	1
35–36	0	0	1	2
33–34	0	0	0	0
31–32	1	6	0	3
29–30	1	1	0	5
27–28		2	1	0
25–26		0		1
23–24		1		5
21–22		1		2
19–20		0		2
17–18		1		2
15–16				0
13–14				1
N:	16	16	24	24
Mean:	45.31	30.50	42.83	26.12
SD:	6.47	5.93	5.32	6.28

The high school had a system of volunteer social service activities, participation in which gave "points" to the student. Thirty-four of the high school students had acquired social service points, and 89 had not. The biserial *r* between this dichotomy and scores on the dominance scale was .33.

Another comparison was made between dominance measures and social status, as defined by an objective inventory (1). The correlation of the Home Index with the

dominance scale was .352, with the nominations .266, and with self-ratings .302, *SE* .09 throughout. (These comparisons were made only in the high school group.)

For the total 60-item dominance scale, the means and standard deviations were 37.06 and 8.47, and 34.11 and 8.06, for the total college and high school samples, respectively.

The 28 items drawn from the MMPI were also considered separately. In the college group, correlation of the 28-item scale with the group ratings was .524, and with self-ratings .651, *SE* .14 throughout. In the high school sample the MMPI dominance scale correlated .597, *SE* .09, with nominations, and .411, *SE* .09, with self-ratings.

The distributions of raw scores on the MMPI portion of the dominance scale for the college and high school samples, and for two additional samples, are presented in Table 3.

The decline in average dominance scores from the older to the younger age groups may be remarked (the *F*-test is significant beyond the 1 per cent level). Two equally likely (not necessarily exclusive) explanations can be advanced for this trend. It may actually be a matter of age and maturity, etc., or it may be that the more ambitious and capable (and hence more dominant) students are more likely to move on to twelfth grade and college from the earlier high school years. *T*-scores for this MMPI dominance "scale" have not been provided because of the highly selected nature of the samples studied. Workers who care to use the scale, however, may compute tentative conversion tables from the data given in the last column of Table 3.

The present study has certain serious limitations in light of the broader purposes for which it was carried out. The most impor-

TABLE 3

Distribution of Raw Scores on the 28 Items of the Dominance Scale Taken from the Minnesota Multiphasic Personality Inventory

Score	Criterion Groups		Additional Samples		Total
	College	High School	High School	Ninth Grade	
28					
27					
26		1			1
25	1	1			2
24	1				1
23	6				6
22	4	3	6	3	16
21	1	7	5		13
20	5	6	11	5	27
19	3	9	14	7	33
18	8	13	14	13	48
17	8	10	29	17	64
16	2	15	24	24	65
15	4	7	23	32	66
14	1	11	29	32	73
13	1	15	30	21	67
12	2	7	28	17	54
11	2	5	27	15	49
10		5	21	17	43
9		5	9	11	25
8			6	2	8
7			4	2	6
6	1	1	1	1	4
5		2	1	1	4
4					
3				1	1
2					
1					
0					
N:	50	123	282	221	676
Mean:	18.08	15.47	14.20	13.98	14.64
SD:	3.84	3.89	3.50	3.17	3.70

tant of these is that the larger research project is being conducted entirely with adults, and the present scale has been developed on the basis of the responses of adolescents only. The extent to which this disparity will obscure and prejudice results when the scale is used on the adult population cannot be predicted, but it may be assumed that some distortion will occur. For one thing, it seems unlikely on the basis of casual observation that the rather stern morality of the dominant adolescents considered here will prevail to the same degree among adults.

In spite of these manifest limitations the authors would conclude that the dominance scale in its present form is a useful measuring instrument and should yield dependable results for the specification of group trends in most situations.

Summary

Dominance is one of the dimensions that would appear to be of importance in the psychology of political participation. It was accordingly decided to attempt the construction of a dominance scale which would reliably predict group judgments of dominance and which would incorporate a wider and more significant range of item content than has been true of previous scales.

A final 60-item scale was evolved which included 28 items from the Minnesota Multiphasic Personality Inventory and 32 original items. This scale correlated to the extent of .599 and .687 with group ratings on dominance, and appeared to be sufficiently reliable for group interpretation.

The implication of a number of item clusters for personality analysis was discussed, and certain limitations of the scale were recognized.

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THE NATURE AND MEANING OF THE RORSCHACH WHITE SPACE RESPONSE [1]

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WHEN Rorschach (23) published his inkblot test nearly thirty years ago, he assumed that space responses always indicate some sort of opposition tendency. At that time, he wrote (p. 200):

> It is striking to note how the study of the space response defines the neurotic aspects of the record. This is frequently the case, though I am as yet unable to say why it should be . . . this will be known only through further experience.

Since publication of the *Psychodiagnostik*, the inkblots have been presented to more subjects than any other single set of stimuli in the history of psychology, and the space responses have been regularly interpreted as evidence of opposition trends. Unanimous endorsement of this practice is found in contemporary Rorschach texts and manuals (1, 2, 3, 12, 16, 19, 21), but such interpretations are still based mainly upon clinical hunches and intuition rather than clear-cut objective evidence. Indeed, Rapaport (22, p. 177) recently felt obliged to draw attention to a "dire need for verification" of the rationale underlying interpretations of the white space response.

The following study was designed to explore several of the assumptions underlying interpretations of the white space response. This involved an examination of scoring techniques and reliability, the compilation of normative data, and a limited assessment of validity.

THEORETICAL CONSIDERATIONS

There appears to be substantial agreement in the Rorschach literature with respect to the theory and practice of space-response interpretation, except as noted below.

Nature of the S *Response*

Rorschach (23, p. 39) defined *S* responses as "those answers in which the white spaces are interpreted rather than the black or colored parts of the figure which surround them." Such responses are customarily included in the location category when a Rorschach protocol is summarized, but, as responses to contrast, they often seem to have characteristics in common with color-determined answers as well. Possibly relevant to this observation is the finding of Lazarus (17), who reproduced the colored blots in black and white and presented a test otherwise identical to the original Rorschach. This change produced an increase in the number of *S* responses, a result which might be better understood if it is assumed that brightness contrast or whiteness determines many *S* answers in a way analogous to the influence of color.

This dual quality of the white space response has led to a variety of scoring procedures, some of which combine area chosen (*W*, *D*, etc.) with the determinant (*S*) in the scoring symbols summarized under the location category. Rorschach (23) did not use such combinations, however, scoring responses like the following (to Card II) simply as *S* (p. 187):

> A wide parkway (space form) lined by beautiful dark trees (black), and it loses itself in the distance in a fence (middle black).

Other writers distinguish two principal types and four sub-categories of *S* responses. These are given in Table 1.

The distinction between main and additional *S* responses has resulted in a lack of agreement among Rorschach workers as to scoring procedures. Beck (1, Vol. I, p. 53), for example, gives equal weight to both main and additional *S* responses, stating that differential weighting merely complicates scoring procedures without adding diagnostic usefulness. Klopfer (16), on the other hand, does not advocate inclusion of additional *S* responses in the scoring summary. Munroe (20) and Bühler and Lefever (5) give a

[1] The author wishes to express appreciation for the helpful advice and encouragement extended by Dr. Richard S. Lazarus during the preparation of this report at The Johns Hopkins University.

TABLE 1

TYPES OF SPACE RESPONSE DESCRIBED BY VARIOUS WRITERS

TYPE OF SPACE RESPONSE	SYMBOLS		
	HERTZ (12)	BECK (1)	KLOPFER (16)
A. *Main*			
1. Large, major, or "normal" white areas	*S*	*Ds*	*S*
2. Small, minor, or "rare" white areas	*s*	*Dds*	*S*
B. *Additional*			
3. Major inked area within which small or large white space is included as a unit, as "Halloween mask"	*D(S), D(s)* *W(S), W(s)* etc.	*Ds*	*D,S* *W,S*
4. Minor inked area seen as a unit with inclusion of minor white space, as "an open mouth"	*Dr(s)*	*Dds*	•

• These might be symbolized as *dd,S*; *de,S*; *dr,S*; or *di,S*; but Klopfer does not discuss this category.

weight of ½ to each additional *S* response in their check lists, but do not explain the logic behind this departure. This problem may be of minor importance for individually administered and interpreted records; but for group administration, for research, and for check list interpretation, better agreement as to scoring procedure would seem imperative.

Meaning of the S *Response*

Rorschach (23, p. 200) postulated the existence of a relatively stable personality disposition which he called a "tendency to opposition" and deduced that "space responses always indicate some sort of oppositional trend." A second postulate asserted the existence of three experience types: introversive, ambiequal, and extratensive. From these two postulates, Rorschach elaborated a series of deductions with regard to the type of behavior he would predict from space responses in each experience type. These deductions, and the predictions based upon them, are presented schematically in Table 2. All contemporary expositions of the meaning of *S* responses are apparently in full agreement with this schema.

Critical Frequency of S *Responses*

How many *S* responses must appear in a record to be considered excessive? Rorschach (23, p. 39) thought that "more than one *S* in a protocol gives reason for suspicion." Beck (1, Vol. II, p. 47), on the other hand, expects healthy subjects to give from 2 to 4 space responses. Bühler and Lefever (5, p. 11), disregarding their own statistical evidence, assert that *S* scores should be at least 3½ for unfavorable significance. Unfortunately, these apparently divergent recommendations are not comparable because, as noted above, each writer advocates a different scoring technique for the *S* response.

TABLE 2

RORSCHACH'S POSTULATES, DEDUCTIONS, AND BEHAVIOR PREDICTIONS

(Related to the White Space Response)

POSTULATED EXPERIENCE TYPE	DEDUCED OPPOSITION TREND	PREDICTED BEHAVIOR	STIMULUS SITUATION
Extratensive	Contrariness	Defiant, stubborn, polemical, contradictory	Wishes, commands, conditions imposed by others
Ambiequal	Indecisiveness	Skeptical, hesitant, doubtful, vacillating	Requests for decisions
Introversive	Feelings of inadequacy	Self-critical, circumstantial, self-distrustful	Requests for self-evaluation

PREVIOUS STUDIES

No experimental research specifically designed to evaluate the *S* response has been reported, but some incidental observations with regard to validity, reliability, and norms have appeared. These will be reviewed briefly here.

Validity

One early study in this area was reported by Boss (4), who examined 75 antisocial psychopaths in a Zurich clinic and ranked them according to deviation from social standards. No statistical data were presented, but Boss considered his results a confirmation of Rorschach's hypothesis. "The more white space responses a subject produces," he wrote,[2] "the greater the evidence for character deviation with respect to social standards." He published the full protocols for seven of his subjects (ranks 1, 6, 25, 38, 45, 63, and 75). *S* scores diminish roughly in reverse order, but so do response totals. Moreover, since Boss ranked his subjects partly on the basis of their Rorschach records, it is difficult to assess the degree of support this evidence gives to Rorschach's hypothesis.

A different kind of study was reported by Cook (6), who tested 50 young native Samoan men, of whom 72 per cent made use of the white spaces. Cook considered this percentage excessive but refused to concede, in the absence of other evidence, that these *S* responses indicated negativistic or oppositional tendencies. As an alternative explanation, he mentioned that white is the Samoan's favorite color, symbolizing superiority. This explanation treats *S* as color-determined rather than as a location response.

Another study deserving mention is reported by Rapaport, who tested 217 hospitalized and 54 nonhospitalized subjects classified into 22 groups on the basis of diagnosis. Considering *S* per cent (main plus additional), he found the highest mean score in a group of 14 patients whose diagnosis was Paranoid Condition. "The oppositional implications of projective delusions are relatively clear," Rapaport (22, p. 178) wrote, concluding that ". . . a marked incidence of clear-cut space responses in a relatively coarctated record is always suggestive of the presence of a paranoid condition." But he also pointed out that, as yet, positive support for the opposition tendency hypothesis comes exclusively from clinical intuition.

More recently, Bühler and Lefever (5) tested 308 psychiatric patients and 30 nonpatients. The subjects were classified into 16 groups on the basis of diagnosis. Twenty-six Rorschach scoring categories were evaluated for their discriminating power and assigned weights on a composite "Basic Rorschach Score" designed to measure the degree of personality integration. Of the 26 items thus evaluated, the *S* score turned out to be one of the most efficient discriminators. Significant differences were found between the mean score for the nonpatients and the means for each of 14 other groups. The mean for the nonpatient—and presumably healthiest—group was 3.1. This was higher in every case than the mean for the comparison group. It is somewhat surprising, therefore, to note that these authors, in assigning scoring weights to the various items in their check list, gave *negative* weight to the item "3½ or more *S*," thus completely reversing the implications of their data. They have acknowledged, however, that some of the empirically derived scoring weights were modified in the light of clinical experience. "Strangely enough," they wrote (p. 46), ". . . normal individuals who are very compliant and eager to be accepted produce many *S*, indicating concealed opposition and independence." This curious contradiction between empirical evidence and clinical judgment highlights the previously mentioned dire need for validation of the *S* response rationale.

Reliability

Rorschach reliability studies have not specified whether a distinction had been made between main and additional *S* scores. Hertz (11) reported split-half individual-administration reliability coefficients (for *S* per cent) ranging from .75 to .87, and Lindner and Chapman (18) obtained coefficients between group-test and individual-retest scores of .54 and .64 (for raw *S* scores). No previous estimate of *S* score reliability based upon alternate forms of the test is available.

[2] Author's translation.

Normative Data

In view of the lack of agreement among Rorschach workers with regard to the scoring of main and additional *S* responses, it would be expected that published studies giving means and standard deviations of *S* scores are not strictly comparable. Inspection of data for college students (10, 13, 14) and for various clinical groups (5, 22) confirms this expectation. Moreover, Rapaport (22, p. 178) has noted a tendency for *S* scores to vary with total *R*, and Cronbach (17) has reviewed a number of researches in which comparison of groups on certain other Rorschach scoring categories merely duplicates information at hand when the response totals are known. For these reasons, the available normative data are not very useful in answering questions about how many *S* responses in a record should be considered excessive.

PROBLEM AND PLAN OF THIS INVESTIGATION

The foregoing discussion has revealed a number of theoretical and practical questions with respect to the nature and meaning of the Rorschach white space response and the limited evidence available concerning them. The proposed methods for seeking some of the empirical answers are outlined below.

Reliability

At the outset, it may be asked if individual differences in tendency to give either main or additional *S* responses, or both, remain consistent over reasonably substantial periods of time. In this connection, the concept of "response set" should be useful. For example, consider "inkblots" and "*S*" as categories of stimulus and response, respectively. Significant reliability coefficients between two alternate inkblot situations would constitute evidence for the existence of relatively stable sets to give either main or additional *S* responses, or both. Fortunately, a parallel series of blots, suitable for this purpose, has been published (9).

There are a number of reasons for preferring the alternate-form method of estimating Rorschach reliabilities. Especially compelling is the fact that ordinary test-retest coefficients are extremely vulnerable to the contaminating influence of memory, while the split-half method is faced with the very difficult problem of dividing the test into equivalent halves. Furthermore, the split-half method yields no information relative to the consistency of response tendencies from one testing session to another.

Normative Data

In order to evaluate the significance of deviant scores, a method is needed for estimating the magnitude and variability of *S* response scores to be expected in a group of healthy adults. Norms based upon data from a college student population should be useful. Since the use of alternate forms means that each subject is tested twice, norms obtained in this way are likely to be truer measures than those derived from a single test.

Because of the possibility that both the expected magnitude and the variability of *S* scores may vary as a function of total *R*, it is necessary to cast normative data into a form which takes into account the total number of all responses. This could be accomplished by computing equations for the regression on total *R* of both the mean *S* scores and their variability. With this information, one could estimate the relative deviation of any obtained *S* score from a norm in terms of total *R*.

Validity

Only after individual differences in white space response set have been shown to be relatively stable, and a method for indicating the significance of deviant scores devised, would it be meaningful to ask whether Rorschach's hypotheses are capable of objective confirmation.

A test of these hypotheses would involve an attempt to estimate the degree of relationship between the white space response set and the predicted personality variables in Table 2. This would require presentation of stimuli designed to elicit various types of oppositional responses in samples of extratensive, ambiequal, and introversive subjects. Selection of appropriate stimuli for this purpose is complicated by a lack of semantic rigor in the description of opposition behavior. In view of the status of current personality research, this difficulty is perhaps unavoidable. Solution of this problem, as attempted in the present study, involved use of the Guilford-Martin Personnel Inventory, and Inventory

of Factors GAMIN. Both are questionnaires comprising, altogether, 336 items which require "yes," "no," or "?" answers.

The tendency to use the "?" answer has been shown to be a fairly consistent response set (8). It seems reasonable to assume that such a response set reflects a more generalized opposition tendency, since it indicates that the subject is either unable or unwilling to give a definite "yes" or "no" answer. In the first case, he would be exhibiting indecisiveness; in the second, contrariness.

Moreover, since the names assigned to some of the Guilford-Martin factors bear a surface resemblance to the terms in Table 2, it might prove interesting to relate scores for Inferiority Feelings (Factor I), Cooperativeness (Factor Co), and Agreeableness (Factor Ag), to the *S* response scores of individuals grouped on the basis of the three Rorschach experience types.

Finally, it is to be noted that the use of an alternate form allows tabulation of responses to *double* the usual number of inkblots. Here again it is necessary to use a measure of white space tendency which is relatively independent of contributions from total *R*. This could be accomplished by assigning to each subject a standard score in terms both of the mean *S* and its variability as predicted from regression equations on total *R*.

PROCEDURE

Two sets of inkblots—the standard Group Rorschach and Harrower's (9) parallel series—were used in this study. In a modification of Sender's (24) method for group administration, which allows subjects to write their responses on mimeographed answer sheets, each slide was projected for 3 minutes in a semidarkened room. Subjects were tested in groups ranging in size from 19 to 41. They occupied identical seats during two 50-minute test sessions separated by intervals ranging from 4 to 14 days.

At the first group session, subjects were supplied with copies of the Guilford-Martin Personnel Inventory and Inventory of Factors GAMIN, and instructed to return them at the second session. They were also told that a private conference would be arranged with each person after the second testing session, during which his test results would be interpreted to him. In these one-hour conferences, a careful inquiry was conducted for scoring the two inkblot protocols.

The subjects were 150 Johns Hopkins undergraduates. Table 3 shows the order of tests, the intervals between them, and some descriptive characteristics of the groups tested.

TABLE 3

ORDER OF ADMINISTRATION, INTERVALS BETWEEN TESTS, AND SOME CHARACTERISTICS OF GROUPS TESTED

SOURCE	*N*		AGE		INTERVAL (DAYS)
	MALE	FEMALE	MEAN	SIGMA	
		Rorschach first, Harrower second			
V	21	0	22.5	2.9	4
G	0	16	25.0	9.0	14
G	25	0	23.3	4.8	14
A	30	0	22.6	3.2	14
Total	76	16	23.2	5.1	
		Harrower first, Rorschach second			
V	19	0	23.0	3.8	4
S	19	0	22.1	2.7	6
A	20	0	21.3	2.2	14
Total	58	0	22.1	3.1	
Total Sample	134	16	22.8	4.5	

V Volunteers from courses in Personnel and Labor Relations and Biological Science Survey.
G General Psychology course given in McCoy College summer session.
A Course in Abnormal Psychology.
S Course in Social Psychology.

RESULTS AND CONCLUSIONS

Analysis of results obtained in this research is divided into three main categories: reliability, normative data, and validity.

Reliability

Reliability coefficients computed for the variables in this study appear in Table 4. Inspection of this table reveals a significant difference between the coefficients for main *S* (.533) and additional *S* (.177). This finding raises a question concerning the justification for scoring additional *S* responses. When main and additional *S* scores are combined, the correlation between the two tests becomes .427; such combination merely reduces the partial correlation of .323 for main *S* between the two tests, holding total *R* constant. Only about 10 per cent of the variance in space response tendency on one test can be predicted from the other.

Reliability of the Rorschach experience-type classification was estimated from a mean-square contingency coefficient, using Kelley's (15) formula and corrections. This coefficient turned out to be .567. For these data, the maximum coefficient would be .816.

TABLE 4

MEANS, STANDARD DEVIATIONS, AND RELIABILITY COEFFICIENTS OF THE VARIABLES

VARIABLE	PSYCHODIAGNOSTIC INKBLOTS							
	RORSCHACH		HARROWER		r_{rh}	TWO TESTS COMBINED		
	MEAN	SIGMA	MEAN	SIGMA		MEAN	SIGMA	r_{tt}*
Total *R*	28.29	11.09	30.14	12.02	.827	58.43	22.01	.905
Main *S*	1.23	1.47	1.23	1.46	.533	2.45	2.50	.696
Add. *S*	0.59	0.78	0.96	1.14	.117	1.55	1.45	.209
Main + Add. *S*	1.82	1.72	2.19	1.99	.427	4.01	3.58	.598
Std. *S*					.323†			.488†
M	3.38	2.30	3.29	2.26	.520	6.67	3.98	.684
Sum C	1.65	1.42	2.43	1.91	.447	4.08	2.84	.618

VARIABLE	GUILFORD-MARTIN INVENTORIES							
	PERSONNEL		GAMIN		r_{gg}	INVENTORIES COMBINED		
	MEAN	SIGMA	MEAN	SIGMA		MEAN	SIGMA	r_{tt}*
"?" Score	14.00	12.52	14.38	12.81	.809	28.38	24.10	.894
Factor Ag	31.65	9.79						(.80)‡
Factor Co	65.21	15.57						(.91)‡
Factor I			33.55	8.49				(.89)‡

* With Spearman-Brown correction.
† Partial correlation holding Total *R* constant.
‡ Reliability coefficients from test manual.

estimated reliability. Moreover, the correlation between main *S* and additional *S* is only .155. This suggests that the two types of *S* score may not be measures of the same thing. It may be said, therefore, that continued use of the additional *S* category appears to be unwarranted. This observation should be of particular interest to those psychologists (e.g., 5, 20) who advocate use of half-weighted additional *S* scores in check list evaluations.

In view of the very modest reliability coefficient for main *S* found in this study, only the more extreme scores should be taken as a basis for individual protocol interpretation. This need for caution in prediction from individual scores is emphasized by the

Experience type was defined as follows: Each subject was classified on the basis of the equation

$$E = \frac{M}{M + Sum\ C},$$

where *M* and *Sum C* have their conventional Rorschach scoring significance and *E* is experience type. The value of *E* varies from 0 to 1. Cases with *E* values between .4 and .6 were classified as ambiequal, below .4 as extratensive, and above .6 as introversive.

The experience-type classifications appear to have limited reliability. In this study, only 57 per cent of the subjects were identically classified by both tests; 30 per cent shifted into or out of the ambiequal category, while

13 per cent switched from extratensive to introversive or the reverse. These findings are consistent with results obtained by Thornton and Guilford (25), who reported that 55 out of 100 college students were identically classified by the odd and even cards in a split-half reliability study using individually administered tests. Harrower (9, p. 30) reported that the *M:Sum C* ratio was "unchanged" in 88 out of 100 cases retested with a parallel series of inkblots, but she does not indicate the magnitude of differences tolerated under the heading of "unchanged." If her results mean that 12 per cent switched from introversive to extratensive or the reverse, all others being considered "unchanged," the present study would provide confirmation.

The relatively great mobility of cases near the ambiequal category suggests that verification of predictions made exclusively for such a group would be exceedingly difficult. This observation should be borne in mind when considering Rorschach's hypothesis as to the significance of *S* responses in different experience types.

Normative Data

Figure 1 shows a graph of the regression equation, computed by the method of least squares, which describes the dependence of main *S* on total *R* scores when the data for all 20 inkblots are combined. The magnitude of this relationship is suggested by a correlation coefficient of .635 between main *S* and total *R*. The part-whole coefficient between *S* and R—S is .597. Moreover, inspection of this figure reveals that a substantial relationship also exists between main *S* variability and total *R*. This condition results in a lack of homogeneity so pronounced that use of the conventional standard error of estimate would be inappropriate. Consequently, it became necessary to compute an equation for the regression of main *S* variance on total *R*. This equation and its graph are also shown in Figure 1.

Figure 2 shows the graph of these equations after adjustment to make them applicable for normative purposes to scores derived from a single ten-inkblot test. Incidentally, the curved line in Figure 2 shows

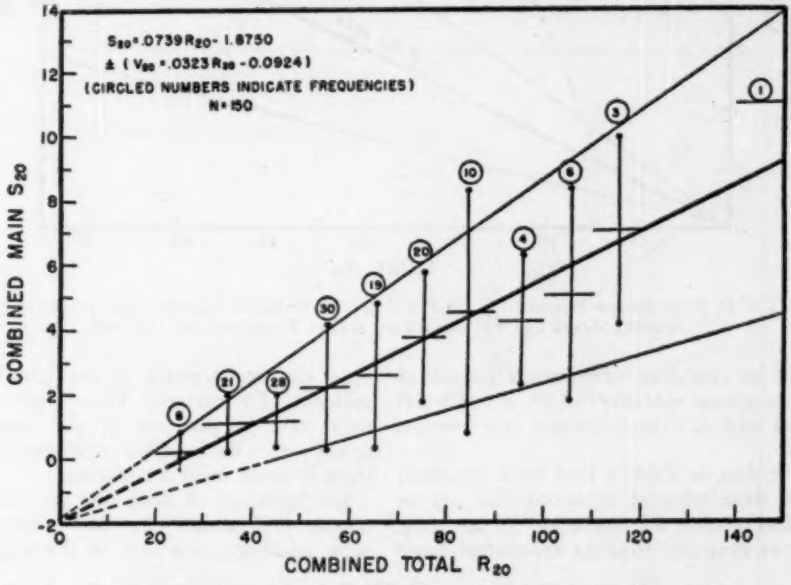

FIG. 1. REGRESSION OF COMBINED MAIN-S_{20} MEANS (HORIZONTAL BARS) AND STANDARD DEVIATIONS (VERTICAL BARS) ON TOTAL R_{20}, WITH GRAPH OF EQUATION FOR $S_{20} \pm V_{20}$ (empirical)

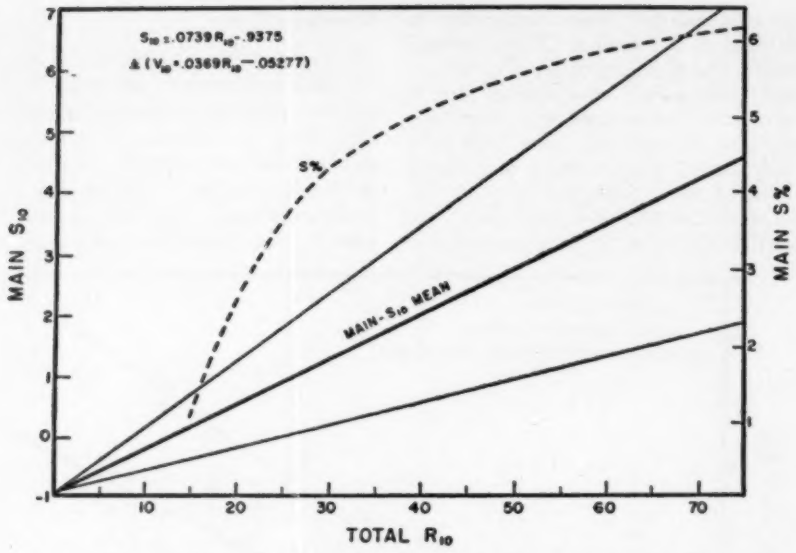

FIG. 2. RELATIONSHIP BETWEEN $S\%$ AND TOTAL R, WITH GRAPH OF EQUATION FOR THEORETICAL MAIN-S_{10} MEANS AND V_{10} VARIABILITY AROUND REGRESSION ON TOTAL R_{10}

that *S per cent* does not provide a measure of space-response tendency which is independent of total *R*, since it changes as a function of *R*.

The data in Table 5 have been computed from these adjusted equations and can be used to estimate the significance of any main *S* score deviation from an expectation based upon the total number of responses to ten inkblots. For example, Table 5 shows that only about 5 per cent of all 30-response records from the sampled population contain three or more main *S* responses.

Interpretations of these data are subject to certain reservations. In this sample, cases were relatively infrequent in the higher re-

TABLE 5

CHART FOR ESTIMATING SIGNIFICANCE OF DEVIANT MAIN *S* SCORES
(Based upon Equations in Figure 2)

WHEN TOTAL *R* IS:	EXPECTED NO. OF RECORDS PER 100 IN WHICH THE MAIN *S* SCORE EQUALS OR EXCEEDS											MEAN S_{10}	VARIABILITY V_{10}
	1	2	3	4	5	6	7	8	9	10	11		
5	0											—	0.14
10	0											—	0.32
15	5	0										0.17	0.50
20	25	2	0									0.54	0.69
25	46	11	1	0								0.91	0.87
30	61	25	5	0								1.28	1.05
35	70	39	14	3	0							1.65	1.24
40	76	50	25	8	2	0						2.02	1.42
45	81	59	35	16	5	1	0					2.39	1.60
50	84	66	45	25	11	4	1	0				2.76	1.79
55	86	72	52	33	17	7	3	1	0			3.13	1.98
60	88	75	59	41	25	12	5	2	1	0		3.50	2.16
65	89	79	64	48	32	18	9	4	1	0		3.87	2.35
70	90	81	69	54	38	24	14	7	3	1	0	4.24	2.53
75	91	88	72	59	44	31	19	11	5	2	1	4.61	2.71

sponse brackets where the relationship between main *S* and total *R* may not be linear. This can be observed by noting the frequencies given in Figure 1. Therefore, estimates based upon records with more than 40 responses can be made with less confidence. Furthermore, interpretations should be made with the characteristics of the sample population in mind. It is to be remembered that observed deviations are from group-test norms established for college students.

Validity

In order to make use of individual differences in *S* response tendency independent of total *R*, each subject was assigned a standard *S* score. This is an index of his deviation from the mean *S* predicted on the basis of equations in Figure 1. Correlations between standard *S* scores and the criterion variables for the three experience types may be found in Table 6. Table 7 gives all the intercorrelations for the entire sample.

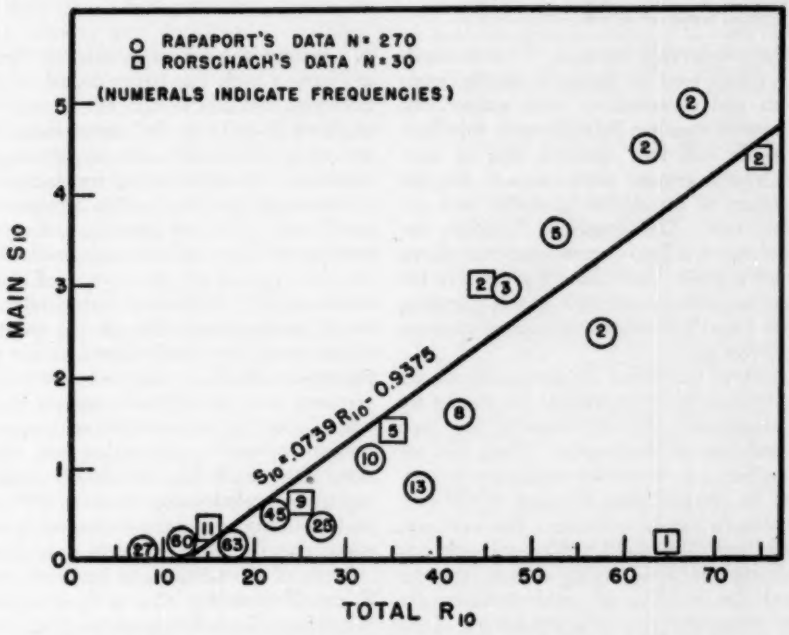

FIG. 3. MEAN S_{10} SCORES WITHIN TOTAL R_{10} INTERVALS FOR 300 SUBJECTS REPORTED BY RAPAPORT AND RORSCHACH

Some notion of the generality of predictions from the regression equation, however, can be gained by inspection of Figure 3, which shows the dependence of main *S* on total *R* in *patient* populations tested *individually* by Rorschach (23) and Rapaport (22). The means presented here were computed by the present author. For comparison with these observations, the regression line from this study is also reproduced. Agreement among the results of the three studies appears to be satisfactory.

Of the criterion variables, only the "?" scores are significantly related to the S response. The correlation between standard *S* and "?" scores for the entire sample is .574 when corrected for attenuation. Without the correction, the correlation is .379. While differences among the experience-type subgroups are well within the range of variable sampling errors, it is virtually certain ($p < .01$, Fisher's z transformation) that the population value of this coefficient lies between .192 and .556. There is a suggestion

TABLE 6

PRODUCT-MOMENT CORRELATION COEFFICIENTS BETWEEN STANDARD *S* SCORES AND OTHER VARIABLES IN THIS STUDY, COMPUTED FOR TOTAL SAMPLE AND FOR RORSCHACH EXPERIENCE TYPES

VARIABLE	EXPERIENCE TYPE			TOTAL (N=150)
	(E>.6) INTROVERSIVE (N=80)	(.6>E>.4) AMBIEQUAL (N=47)	(E<.4) EXTRATENSIVE (N=23)	
"?" Score	+.415*	+.253	+.426**	+.379*
Factor Ag	−.093	−.218	−.172	−.142
Factor Co	+.004	−.051	−.334	−.090
Factor I	−.126	−.090	−.130	−.133

* Significant beyond the .01 level.
** Significant beyond the .05 level.

that the relationship between "?" and standard *S* scores may be stronger among introversives and extratensives than ambiequals. If "?" scores measure indecisiveness, this finding would call into question one of Rorschach's hypotheses with respect to the interaction of opposition tendency and experience type. Unfortunately, however, the relative unreliability of experience-type classifications deprives these data of a capacity for yielding a definitive answer to this question, provided that "?" scores do, indeed, measure indecisiveness.

Consistent individual differences in the set which produces "?" responses are shown by the correlation of .809 between the two Guilford-Martin inventories. That this set may reflect an opposition tendency is suggested by consideration of some of the possible reasons for its existence. For example, during their Rorschach conferences subjects frequently displayed highly critical attitudes toward the wording of some questionnaire items. Many subjects declared that use of the "?" answer was an expression of their dissatisfaction with the ambiguity of offending questions. Excessive use of the "?," then, might well point to the contrariness which, according to Rorschach's hypothesis, is a manifestation of opposition tendencies.

The significant relationship between standard *S* and "?" scores provides evidence that predictions from at least one level of Rorschach's hypotheses are capable of objective confirmation. This is of considerable theoretical interest, even though the magnitude of the correlation coefficients does not justify practical prediction. The fact that these two response sets are related suggests that "?" scores may become convenient intermediate criteria for use in personality test construction. One study (8) has already reported a significant relationship between "?" scores and the more ultimate criterion of "having a reputation for being an 'autocratic' teacher."

None of the correlations between standard *S* and Factors Ag, Co, or I, is significant. Therefore, the null hypothesis (that there is

TABLE 7

INTERCORRELATIONS AMONG VARIABLES (COMBINED DATA, BOTH TESTS) AND TEST-RETEST COEFFICIENTS (N=150)

VARIABLE	*R*	MAIN *S*	STD. *S*	?	Ag	Co	I
Total *R*	.827	.635	−.080	−.051	−.070	−.024	.094
Main *S*		.533	.638	.087	−.182	−.102	.014
Std. *S*		.895*	.323*	.379	−.142	−.090	−.133
"?" Score				.809	−.167	−.171	−.429
Factor Ag					(.80)†	.620	.232
Factor Co						(.91)†	.474
Factor I							(.89)†
Add. *S*		.155					
R—*S*		.557‡					

* Partial correlation holding Total *R* constant.
† Split-half coefficients from test manual.
‡ Part-whole correlation.

no relationship between these variables) cannot be rejected. But because of the unknown validity of the Factor scores, the low magnitudes of their correlations with standard *S* contribute no useful information relevant to the purposes of this study.

SUMMARY

Two personality questionnaires and two inkblot tests were given to 150 college students. Relatively consistent sets to produce certain types of response in each of the tests were observed. These sets were: (*a*) in the inkblot tests, to give responses to the white spaces on the blots; (*b*) in the questionnaires, to make frequent use of the "?" answer. The tendency for these two sets to be associated in the same individuals was found to be highly significant. This finding is presented as partial confirmation of Rorschach's hypothesis that white space responses indicate some sort of opposition tendency, insofar as the criterion measure reflects indecisiveness or contrariness.

No relationship was found between white space responses and such personality factors as Agreeableness, Cooperativeness, or Inferiority Feelings, as defined by scores on the Guilford-Martin inventories.

The intimate dependence of "main" space response scores (complete figure-ground reversals) upon response totals was evaluated, and normative data were presented.

Computation of reliability coefficients between the two inkblot tests led to two recommendations: (*a*) the combination of "main" and "additional" space response scores in protocol summaries should be discontinued; and (*b*) interpretations of the "main" space response should be limited to extremely deviant scores.

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RESPONSES OF PSYCHIATRIC PATIENTS TO THREAT OF FAILURE [1]

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THE present investigation is concerned with the comparative reactions to a situation threatening serious failure on the part of subjects (*Ss*) with various types of personality disorder. It is assumed that pathological personality traits are adjustive responses to the "most painful" problems of modern life: those which occur in "relations with other men" (8). In our society, some of the most ubiquitous interpersonal conflicts are concerned with the competitive struggle for success.

As a result of countless life experiences, the adult learns to expect the approval of significant others for the successful pursuit of socially recognized goals. And conversely, he tends to equate failure with disapproval and loss of self-esteem. To the victor in every field go the spoils. Even if a person is second in a very large group, his interiorized standard of success often leads him to make the invidious comparison between his performance and that of the leader.

How might striving for success be related to personality disorder? It would seem that at least two factors are involved. The first is the increasing tendency of modern man to judge himself in terms of others' evaluations of the success and failure of his activities. As Fromm (9) has observed, one of the primary social changes in the last few centuries has been the gradual disappearance of the absolute values of the Middle Ages. As a result of the greater communication afforded by the industrial era, these standards have been replaced by more secularized ones which revolve about the ego as a central value. Thus, for example, when an individual pursues an important goal, success is a primary yardstick for the evaluation of the self. Failure entails a lowering of self-esteem.

[1] The author is greatly indebted to Drs. Maud Merrill James, Robert E. Harris, and Howard F. Hunt for their many suggestions, to Mrs. Elaine W. Barron, Mr. Stuart Axtell, and Dr. C. L. Winder for their assistance in administering the tests, and to Dr. Gerald S. Blum for his critical reading of the manuscript. The research was facilitated by a Social Science Research Council Pre-Doctoral Fellowship.

The second factor in the relationship between goal-striving and personality disorder seems to be the raising of standards by which success is socially defined. Definitions of success, as well as evaluations of failure, vary greatly among the different social, economic, geographic, and ethnic groups of American society. But common to all groups is the increasing tendency to define their goals in terms which make excellence the achievement of the few, and for those few only in limited areas.

When failure is intense or prolonged, it may lead to unbearable anxiety. Subsequent adjustments may require pathological alterations of the personality. This leads to the hypotheses of the experiment: (1) at one level, many symptoms of current personality disorders are meaningful primarily if interpreted as defenses against failure; (2) and since various types of personality disorder require different defenses, *Ss* with separate syndromes should respond differentially to threat of failure. The data presented in the remainder of this paper refer primarily to the second hypothesis.

METHOD

The specific reaction to threat of failure investigated in the present experiment was the level of aspiration. This is the "level of future performance in a familiar task which an individual, knowing his past performance in the task, explicitly undertakes to reach" (7, p. 119).

Instructions were designed with two purposes in mind. The first was to define the situation in such a way that it would have a similar meaning for all *Ss*. To prevent differential interpretation of test conditions by various groups, an attempt was made to eliminate all cues which might be of aid in the accurate guessing of actual test performance. An attempt was made to minimize similarity to other tasks by using an unusual three-dimensional formboard, the Carl Hollow Square Scale. The *S* was kept in ignorance of his relative standing in the group, since level of aspiration partly depends upon knowledge of one's ability (14). No information was given about upper and lower scoring limits, or the scores of other *Ss*. Each individual was told that scores were computed by comparing his times with those of the other *Ss*. Since a change in score

might have resulted from shifts either in his performance or in the performances of others, it was impossible for him to know how well he was doing.

Any remaining possibility of interindividual comparison was eliminated by the use of a prearranged set of achievement scores (10, 13, 15). After each trial, all *Ss* were told the same score regardless of their actual performances. This aspect of the design, in addition to insuring the same amount of success and failure for all individuals, was also intended to prevent the *Ss* from assuming improvement with practice when they estimated their levels of aspiration.

The second purpose of the instructions was to have *S* perceive the task as an important life situation, not as a trivial exercise. Ego-involvement was facilitated in three ways:

1. The Carl Hollow Square Scale is deceptively simple in appearance. Many *Ss*, upon first seeing four congruent right triangles which were to be fitted into a square frame, set a premature aspiration of approximately fifteen seconds. After two minutes of frustrated effort, the intense involvement was readily apparent.

2. The *Ss* were selected from voluntary hospital admissions who seemed eager to receive help for their complaints. Inasmuch as the administration of the level of aspiration test typically coincided with the hospital battery of psychiatric and psychological techniques, the *Ss* became quite concerned with test results.

3. The *Ss* were informed that (*a*) this was "one of the most critical tests" they would be given, (*b*) that it would indicate the amount of progress they might be expected to make in the near future, and (*c*) that the results would be considered in planning treatments. Both therapists' accounts and introspections of *Ss* bear testimony to intense ego-involvement in all groups.

In order to allay anxiety afterward, the test situation was discussed with each *S* in a final interview. At that time, he was told that the test was intended to measure his reactions to a difficult situation. After being informed that he had done very well, he was given an opportunity to express his feelings. Finally, he was asked not to discuss the test with other patients because "We give the test in the interest of those being helped here, and the best results are gotten when patients have no idea of what the test is like." Therapists' reports and subsequent interrogation of patients revealed no instance in which this request had been violated.

Selection of Subjects

The *Ss* were obtained from the Langley Porter Clinic, University of California Hospital, and Palo Alto Veterans Administration Hospital. There were four experimental groups: conversion hysterics, neurasthenics, character disorders, and paranoid schizophrenics. In addition, there was a control group of normal *Ss*. Each of the five groups contained 25 *Ss*. The five groups were equated for age, sex, education, and ward vs. outpatient status.

Clinical diagnosis. The choice of *Ss* was based on clinical diagnosis and on results from the Minnesota Multiphasic Personality Inventory and the Rorschach test. Clinical categories were so defined as to facilitate the selection of relatively pure representatives of each syndrome. Because the definitions of syndromes differed slightly from the classificatory systems in use in the hospitals, case histories of potential *Ss* were reclassified by staff members according to a list of criteria supplied by the writer. Conversion hysteria, for example, was identified clinically by the presence of a conversion syndrome which was judged to represent a symbolic expression of a well-defined emotional content. It was also necessary that the conversion be expressed by the voluntary neuromuscular or sensory-perceptive systems (1). Additional criteria of conversion hysteria included evidence of general body erogeneity and "replacement of real sexual objects by fantasy representatives of infantile objects" (5, p. 216).

The clinical diagnosis of neurasthenia was based on the nonsymbolic expression of nervous energy in the form of such vegetative symptoms as sweating, tremor, and diarrhea.

The primary criterion of character disorder was that the entire personality be so involved in the illness that "there is no border-line between 'personality' and 'symptom' " (5, p. 464). It was necessary that the character symptom reflect generalized modes of behavior also discernible in expressive movements such as gait, expression, and posture. An additional criterion was the need of the patient to make plausible rationalizations of socially handicapping symptoms. Examples were offered the raters of impulsive, schizoid, and compulsive characters.

In order to minimize the possibility of obtaining disoriented and deteriorated individuals in the schizophrenic sample, only paranoid cases were selected. The primary criterion was the presence of clinically verified delusions.

Normal patients were obtained from nonpsychiatric wards and outpatient departments of the University of California Hospital. Clinical identification was based on the resident's opinion that the patient was apparently well adjusted and cooperative and that his complaints were not psychogenic. Because of the limited psychiatric training of the residents, the normals were probably the least accurately diagnosed of the five groups.

Analysis of Test Results

Minnesota Multiphasic Personality Inventory profiles and Rorschach protocols were classified blindly according to the criteria listed in Table 1.

The patient was included as an *S* if his clinical diagnosis agreed with at least one of the two tests. There was the further restriction that the remaining test not disagree markedly with the other criteria. For example, if both clinical diagnosis and MMPI yielded a label of hysteria, the case was accepted only if some form of neurosis was indicated by the Rorschach test. More than twice the 125 required *Ss* were tested in order to obtain a sufficient number of *Ss* who met the combined criteria. In 104 of the accepted cases, all three diagnostic instruments were in perfect agreement.

TABLE 1

CRITERIA FOR ANALYSIS OF TEST RESULTS

GROUP	MMPI *	RORSCHACH TEST †
Conversion Hysteria	*Hy* should be above 70 *Hs* must be at least 5 points less than *Hy* when corrected by *K* *Hy* may be exceeded by *Mf* or *Pd*, but preferably not the latter *Sc*, *D*, *Ma*, and preferably *Pa* should be below 70	15 responses or less *A*% above 50 No more than 1 *Fc* or *Fk* Some *CF*; it should exceed *FC* High *P* Stereotypy Anatomy responses
Neurasthenia	*Hs* and *D* should be above 70 *Hy* must be at least 5 points less than *Hs* *Pa*, *Sc*, and *Ma* should be below 70 The other tests may be above 70	Marked indication of anxiety: *fm*, *dd*, *Fk*, *F*%, etc. Harrower-Erickson's signs
Character Disorder	Neurotic and psychotic triads should both be above 70 The neurotic triad should be higher *Mf* should be less than 70	The record should contain predominantly neurotic signs such as very high or very low *R*, excess of *CF* over *FC*, etc. It should not be a psychotic record, but should contain "psychotic-like" signs such as contamination tendency, "eyes" in the content, and possibly 1 *O*—
Paranoid Schizophrenia	*Sc* must be above 70 *Pa* should be above 70 With *K* correction, peaks should be at *Sc* or *Pa* An additional peak at *D* is acceptable	Presence of psychotic signs such as *DW*, *O*—, contamination, *Po*, etc. Paranoid profile: high *M* and *F* and excess of *FC* over *CF*; absence of neurotic signs
Normal	All scores except *Mf* must be below 70	"Normal" record; absence of signs for various pathological groups; not too constricted

* The author is indebted to Dr. R. E. Harris for guidance in the use of his unpublished norms for the analysis of patterns and subscales of the Minnesota Multiphasic Personality Inventory, and for his many discussions of the literature.

† The choice of criteria for scoring the Rorschach test was based on agreement among authorities. Representative of sources consulted are the texts of Beck (3), Bochner and Halpern (4), Klopfer (18), and Rapaport (21) and miscellaneous articles such as Beck's (2) on schizophrenia and Kemple's (17) on psychosomatic diagnosis.

RESULTS

It is hypothesized that since various types of personality disorder require different defenses, *S*s with separate syndromes, when threatened with failure, will respond differentially to a level of aspiration situation. This section contains the analyses of performances of the five groups.

Goal discrepancy. This measure is the difference between the *S*'s score on one trial and his level of aspiration for the next trial. For example, if he got 6 points and estimates that he will get 8 on the next trial, his discrepancy score is plus 2.

In computing the central tendency of an individual *S*'s goal discrepancies, the writer followed Heathers's (13) practice of using the median instead of the mean. This was done to avoid disproportionate weighting of unusually large discrepancy scores. Means were used to summarize group tendencies because they lent themselves to further required statistical manipulation. Table 2 contains the mean *D*-scores and their standard errors for the five groups.

An analysis of variance yielded an F ratio of 21.16, which is significant beyond the 1 per cent level. Consequently, differences among group means were calculated. Table 3 lists these differences and their significances.

All differences are very significant except two: those between character disorders and conversion hysterics, and between normals and paranoid schizophrenics. On the basis of these differences, the goal discrepancy dis-

TABLE 2

MEAN GOAL DISCREPANCIES

GROUP	MEAN GOAL DISCREPANCY	σ_M
Neurasthenics	1.56	0.11
Paranoid Schizophrenics	0.84	0.25
Normals	0.58	0.11
Character Disorders	—0.36	0.27
Conversion Hysterics	—1.01	0.29

TABLE 3

DIFFERENCES BETWEEN MEAN GOAL DISCREPANCIES

GROUPS	DIFF.	t
Neurasthenics and Conversion Hysterics	2.57	8.43**
Neurasthenics and Character Disorders	1.92	6.62**
Paranoid Schizophrenics and Conversion Hysterics	1.85	4.83**
Normals and Conversion Hysterics	1.59	5.13**
Paranoid Schizophrenics and Character Disorders	1.20	3.23**
Normals and Neurasthenics	0.98	6.33**
Normals and Character Disorders	0.94	3.24**
Paranoid Schizophrenics and Neurasthenics	0.72	2.61**
Character Disorders and Conversion Hysterics	0.65	1.65
Normals and Paranoid Schizophrenics	0.26	0.93

** Significant at the 1 per cent level or less.

tribution can be divided into three levels: (1) the mean score of the neurasthenics is significantly higher than the discrepancies of all other groups; (2) next in order are the low positive mean discrepancies of the normals and paranoid schizophrenics; and (3) at the low end of the continuum are the negative means of the character disorders and the conversion hysterics.

The three obtained levels of goal discrepancy are similar to those found by Gould (11), Gruen (12), and Sears (23), who have labelled them "wishful," "practical," and "self-protective" in that order.

These labels seem to describe accurately the orientations of the three levels. When predicting his next score, *S* had little basis for estimating how well he would perform in comparison with the other *S*s, or even whether his score would go up or down. The most probable score was either the immediately preceding one or an average of all previous scores. In other words, the most realistic average goal discrepancy was zero. Closest to this most probable mean are the normal group's discrepancy of .58, the .84 score of the paranoid schizophrenics, and the −.36 of the character disorders. But paradoxically, the latter discrepancy, though most realistic, is pathological in American society. The prediction of decreased performance violates strongly intrenched social values. Hence, the character disorders cannot be linked with the normals and paranoid schizophrenics, but must be classified as a defeatist group.

Compared to the goal discrepancies of the realistic groups, the improbably high positive neurasthenic aspirations indicate a wishful approach to estimates of future achievement. On the other hand, a self-protective or defeatist outlook seems to characterize the negative aspirations of conversion hysterics and character disorders. These groups, on the average, tend to anticipate that they will achieve less than they have already accomplished. It may be noted parenthetically that the mean goal discrepancy of the character disorders would be lower if it were not for one exceptional, high positive score.

Most investigators of levels of aspiration have postulated that, as a result of cultural pressure, there is "a very strong tendency . . . for the estimates to be higher than the performance" (11, p. 110). There is some evidence of this tendency in Table 2. It is more marked when the discrepancies of the various syndromes are classified by sign alone rather than by magnitude. In Table 4 the groups are ranked according to the numbers of *S*s with positive median discrepancies.

In the groups which conform to the cultural norm, 80 to 100 per cent have positive *D*-scores. Even in the defeatist groups, 24 per cent and 40 per cent of the members have positive discrepancies. A chi-square of 15.93 is significant at the 1 per cent level. On the assumption that the normal group is representative of our culture, differences were

TABLE 4

POSITIVE MEDIAN GOAL DISCREPANCIES

GROUP	N	PER CENT	PER CENT DIFF. FROM NORMALS	CR
Neurasthenics	25	100	+12	1.71
Normals	22	88	—	—
Paranoid Schizophrenics	20	80	−8	0.77
Character Disorders	10	40	−48	3.50**
Conversion Hysterics	6	24	−64	4.00**

** Significant at the 1 per cent level or less.

TABLE 5

NUMBER OF SUBJECTS SHOWING FIVE OR MORE ZERO DISCREPANCIES

GROUP	*N*	PER CENT
Normals	15	60
Character Disorders	10	40
Conversion Hysterics	7	28
Paranoid Schizophrenics	5	20
Neurasthenics	4	16

calculated between that group and the others. Differences between normals and both defeatist groups are significant at the 1 per cent level. On the other hand, the normals do not differ significantly from the other conforming groups.

Zero goal discrepancies. In terms of mean goal discrepancy, three of the five groups tend to conform to the cultural value of getting the "highest score of all" (11). If this were the only determinant, however, aspirations in our society would rise infinitely. The limiting factor, as originally noted by Hoppe (16), is the increasing probability of failure as the level of aspiration rises. And failure is penalized drastically.

It has been shown that conversion hysterics and character disorders defend themselves against failure by predicting a decline in their scores. Neurasthenics predict that they will recoup their losses with great increases. Table 5 casts further light on the reactions of the normals to threat of failure. Contained in that table are the number of *Ss* in each group showing five or more zero goal discrepancies. This is the number at the seventy-fifth percentile of the total distribution of zero discrepancies. That percentile was chosen arbitrarily in order to compare the five groups with regard to numbers of *Ss* who had obtained the most zero discrepancies.

Thus it can be seen that the normal group is the only one in which the majority predict at least 5 out of 19 times that they will get the same score as the one just attained. The chi-square of 9.62 reveals that the over-all differences in Table 5 are significant at the 5 per cent level. A comparison of the percentage of normals with the combined percentage of the other groups yields a critical ratio of 3.09, which is significant at the 1 per cent level.

The marked sensitivity of the normals to threat of failure is shown even more clearly in Table 6 which ranks the groups not by medians but by number of *Ss* with a majority of positive goal discrepancies.

Like the defeatist groups, most normals predict they will do the same or worse in most of their levels of aspiration. The chi-square of 23.09 yields a chance probability of less than 1 per cent. Differences between normals and all but the character disorders are significant at the 5 per cent level or better.

The median goal discrepancies of 22 of the 25 normals are positive. But the majority of the individual discrepancies in the case of 15 of the 25 are not positive. When their achievements diminish, the normals are able to lower their aspirations and predict that they will do the same or even that they will continue to get worse. But when they seem to be improving, they can increase their predictions considerably so that they establish a positive average of all their goal discrepancies.

In this respect, the normals seem to differ fundamentally from the paranoid schizophrenics. Tables 5 and 6 reveal that the paranoids are much less able to shift from a positive to a zero or negative discrepancy

TABLE 6

NUMBER OF SUBJECTS SHOWING TEN OR MORE POSITIVE GOAL DISCREPANCIES

GROUP	*N*	PER CENT	PER CENT DIFF. FROM NORMALS	*CR*
Conversion Hysterics	2	8	−24	2.12*
Character Disorders	3	12	−20	1.70
Normals	8	32	—	——
Paranoid Schizophrenics	15	60	+28	1.99*
Neurasthenics	24	96	+64	4.00**

* Significant at the 5 per cent level or less.
** Significant at the 1 per cent level or less.

when such a change seems indicated by the situation. In general, they tend to predict small positive gains irrespective of previous attainment. The lack of significance between the average discrepancies of paranoid schizophrenics and normals thus masks a dissimilarity in their abilities to adjust their levels of aspiration to a continuously changing situation.

The normals also differ from the remaining psychiatric groups in their ability to shift their aspirations in both positive and negative directions. Regardless of the trend of their achievement scores, the neurasthenics rigidly predict continuous marked improvement. To avoid failure, the defeatist groups tend to restrict their predictions to negative and zero discrepancies.

Reaction time. Another variable in which the normals tend to differ from the psychiatric syndromes is the time between the notification of previous score and the verbalization of the prediction. The data are contained in Table 7.

The variances appeared heterogeneous, a suspicion which was confirmed by the application of Bartlett's test for homogeneity of variance (19). Consequently, the differences between normals and the other groups were computed by means of t tests for samples of heterogeneous variances. Fisher and Yates tables (6) reveal that only the paranoid schizophrenics and character disorders differ significantly from the normals.

The theoretical significance of these results derives from the common use of delayed reaction time as an operational definition of conflict. It is assumed that in general, the longer the S takes to make up his mind, the more he is stalled by conflicting impulses.

As judged by reaction time, the normals are significantly less handicapped by the conflicts aroused in a highly ego-involved situation than are the non-neurotic psychiatric patients. It seems probable that the lack of significance between mean reaction times of normals and the neurotic groups results from the extremity of the defenses which the latter employ against failure. Their decision times are not lengthened by complicated estimates of probability or fearful anticipations of the consequences of unrealistic aspirations, since they are prepared, even before they learn their achievement scores, to set either extremely high positive or negative discrepancies.

TABLE 7

MEAN REACTION TIMES
(In Seconds)

GROUP	N	MEAN	σ_M	DIFF. FROM NORMALS	t
Paranoid Schizophrenics	24	14.07	1.30	4.96	3.31**
Character Disorders	21	13.52	1.85	4.41	2.21*
Neurasthenics	20	12.86	1.78	3.75	1.94
Conversion Hysterics	20	11.40	1.24	2.29	1.58
Normals	25	9.11	0.75	——	

* Significant at the 5 per cent level or less.
** Significant at the 1 per cent level or less.

It should be noted that the paranoid schizophrenics have the longest reaction time of all the groups. It requires prolonged conflict, or at the least, deliberation, for them to approximate the low positive mean goal discrepancy of the normals.

Inordinately large discrepancies. Another criterion of excessive emotionality is the presence of inordinately large goal discrepancies. In view of the fluctuations of the prearranged attainment scores, any discrepancy as large as plus or minus 5 was arbitrarily defined as unrealistic. It was possible that a realistic S might have predicted one or two extreme discrepancies in the course of exploring the properties of the task. Therefore a cutting point of three or more deviant predictions was established. The number in each group falling in this category is presented in Table 8.

The chi-square of 6.20 has a chance proba-

TABLE 8

NUMBER OF SUBJECTS SHOWING THREE OR MORE LARGE DISCREPANCIES

GROUP	*N*	PER CENT
Character Disorders	15	60
Conversion Hysterics	11	44
Neurasthenics	10	40
Paranoid Schizophrenics	10	40
Normals	4	16

bility of 18 per cent. Hence, although the percentage difference of 46 between normals and the combined psychiatric groups is significant at the 1 per cent level, only a trend can be claimed for psychiatric patients to set the more extreme goal discrepancies. The inordinate fluctuations and longer reaction times indicate more conflict and emotion in the psychiatric groups.

Attainment discrepancy. Thus far, the primary source of data has been the goal discrepancy score. A measure which contributes further information concerning cultural pressure and differential defenses against failure is the attainment discrepancy. While the goal discrepancy is the difference between the score on one trial and the prediction for the next one, the attainment discrepancy is the difference between the prediction for the next trial and what the person then actually attains in that trial. In other words, the attainment discrepancy indicates the accuracy of the prediction. For that reason, it is often used as an operational definition of *S*'s experience of success or failure. If the discrepancy is positive—if *S*'s achievement surpasses his estimate—the assumption is made that he feels he has succeeded on that trial. If the attainment discrepancy is negative, a failure experience is postulated, since *S* has not reached his level of aspiration.

If the final median attainment discrepancy is positive, it is assumed that *S* has been sufficiently concerned with avoiding failure so as to set a limit to his aspirations. If the median discrepancy is negative, *S* must have been much more concerned with improvement than he was with avoiding failure.

Table 9 compares the mean attainment discrepancies of the five groups.

The analysis of variance yields an *F* ratio of 11.51, which is significant at the 1 per cent level. Table 10 contains an analysis of the differences among the means.

TABLE 9

MEAN ATTAINMENT DISCREPANCIES

GROUP	MEAN DISCREPANCY	σ_M
Conversion Hysterics	2.23	0.36
Character Disorders	1.61	0.29
Normals	1.11	0.17
Paranoid Schizophrenics	0.76	0.25
Neurasthenics	—.11	0.16

The only nonsignificant differences are those between the four adjacent non-neurasthenic groups in Table 9: between conversion hysterics and character disorders; between character disorders and paranoid schizophrenics; and between paranoid schizophrenics and normals. However, all of these groups have positive mean attainment discrepancies which differ significantly from the negative discrepancy of the neurasthenics. In terms of the definition of attainment discrepancy, the four groups experienced success while the neurasthenics felt they had failed. From another point of view, the need to avoid failure places a much lower limit on the achievement needs of non-neurasthenic groups than on those of the neurasthenics. This cautiousness, both in normals as well as members of three psychiatric syndromes, probably reflects a fear of the extreme penal-

TABLE 10

DIFFERENCES AMONG MEAN ATTAINMENT DISCREPANCIES

GROUPS	DIFF.	*t*
Conversion Hysterics and Neurasthenics	2.34	5.88**
Character Disorders and Neurasthenics	1.71	5.14**
Paranoid Schizophrenics and Conversion Hysterics	1.47	3.30**
Normals and Neurasthenics	1.22	5.15**
Normals and Conversion Hysterics	1.12	2.77**
Paranoid Schizophrenics and Neurasthenics	0.87	2.89**
Paranoid Schizophrenics and Character Disorders	0.85	2.19*
Conversion Hysterics and Character Disorders	0.62	1.35
Normals and Character Disorders	0.50	1.47
Normals and Paranoid Schizophrenics	0.35	1.13

* Significant at the 5 per cent level or less.
** Significant at the 1 per cent level or less.

ties and loss of self-esteem which are the consequences of failure in our society.

Despite this cultural pressure, the neurasthenics predict such unrealistically high scores that failure is almost a certainty. Yet even this group is very sensitive to failure. Their mean is barely negative, and 10 of the 25 *Ss* have positive though uniformly low median attainment discrepancies. Apparently the neurasthenic group places more value upon high aspiration leading to failure than upon low aspiration which can be attained successfully. Some of these *Ss* report that even when they fail they obtain some gratification from having aspired to socially approved high goals. But they admit that such secondary gains do not compensate for the intense anxiety which often follows failure.

CONCLUSIONS AND SUMMARY

It was assumed that a meaningful interpretation of the various psychiatric syndromes could be made in terms of defenses against threat of failure. From this general principle the primary hypothesis of the experiment was deduced: patients with different syndromes should react differentially to an ego-involving situation which seriously threatens failure. To implement this hypothesis, four nosological groups and a normal control group were administered a level of aspiration test under conditions calculated to maximize ego-involvement. The sequence of scores was prearranged. In addition, the *Ss* were kept ignorant of the upper and lower scoring limits, as well as their comparative standing, in order to isolate the emotional component of the level of aspiration. Following are the results found in each of the five groups:

1. *Neurasthenics.* Their mean goal discrepancy is significantly higher than that of all other groups. So high are the aspirations of the neurasthenics that on the average they fail to attain their goals. Thus their mean attainment discrepancy is negative.

This group finds it difficult to predict anything but improvement. It has the fewest *Ss* with more than five zero discrepancies, and it has more *Ss* with a majority of positive goal discrepancies than any other group.

Compared to those of the normals, the neurasthenics' predictions are somewhat more conflicted. Their mean reaction time is not significantly longer, but they have a considerable number of inordinately large goal discrepancies. Apparently, neurasthenics can gain little self-esteem from achieving reasonably low goals. Some of them report secondary gains from setting improbably high goals even when they fail to achieve them. When this outlook is applied to daily activities, the inevitable result is frequent failure. But secondary gains from this striving for inordinately high goals cannot compensate for the intense anxiety engendered by the many failures. Thus, the neurasthenic is the victim of a vicious cycle. High aspiration increases the probability of failure which leads to increased anxiety. To relieve the anxiety the neurasthenic aims for greater successes. He raises his aspiration, which in turn increases the probability of failure and begins the cycle anew.

2. *Paranoid Schizophrenics.* The low positive mean goal discrepancy of the paranoid schizophrenics is significantly different from the most probable score of zero. In that sense the group is overly optimistic. So are the normals, whose mean discrepancy does not differ significantly from that of the paranoid schizophrenics. However, with this variable, the similarity between the two groups ends.

The paranoid schizophrenics are comparable to the neurasthenics in their devotion to high attainment. Both groups are unique in that their *Ss* have a majority of positive goal discrepancies. The comparatively few zero discrepancies in the paranoid schizophrenic group reveals how rigidly they set their goals. In short, when the reality situation requires prediction of the same or a lower score, the schizophrenics still feel compelled to estimate some improvement. Considerable conflict is entailed in these predictions. The schizophrenics' mean reaction time is longest of all the groups. In addition, they have a considerable number of inordinately large goal discrepancies.

Insofar as their mean attainment discrepancy is positive, the paranoid schizophrenics succeed in fulfilling their levels of aspiration. But while they predict a higher mean score than the normals, they actually attain less than the normals. Whereas the normals

adjust their aspirations to the curve of attainment, the paranoid schizophrenics confine themselves to the relatively rigid prediction of small amounts of improvement regardless of their achievement trend.

3. *Character Disorders.* The character disorders, since their mean goal discrepancy is closest to zero, appear to be the most realistic group. This is also indicated by the fact that among the psychiatric groups the character disorders have the most zero discrepancies. Despite this ability to test reality, however, they show many indications of maladjustment.

This group reveals considerable conflict and anxiety in the face of possible failure. Its mean reaction time is second highest, and it exceeds all other groups in number of *Ss* with more than two inordinately large goal discrepancies. When asked about the latter, these *Ss* report being constantly plagued by fears of predicting unrealistically high scores. To compensate for this possibility, they sporadically estimate large drops in their levels of aspiration.

In short, the character disorders are more capable than all other groups of estimating the probability of future attainments. But their extreme fear of failure leads them to predict lower scores than those they think they can attain. This may explain their predilection for zero and negative discrepancies particularly. They seem to be so fearful of failure that they cannot risk predicting improvement even when such a prediction is warranted.

4. *Conversion Hysterics.* This is the most defeated group. The mean goal discrepancy is lowest; it has the fewest *Ss* with positive median goal discrepancies, and the fewest with a majority of positive individual goal discrepancies.

The low number of zero discrepancies estimated by conversion hysterics points to a relatively rigid adherence to the prediction of reduced scores. The extent of emotional involvement aroused by the task is indicated by the number of *Ss* with more than two inordinately large goal discrepancies.

The hysterics' low negative estimates of future accomplishment are such a guarantee of success that they are apparently less worried about failing than are the other pathological groups. This may explain why their mean reaction time is so close to that of the normals. Since the conversion hysterics consistently predict that their scores will go down, they seldom fail to exceed their predictions.

In terms of the values of our society, the "success" of the hysterics is an empty accomplishment, similar to a success experience from tying one's shoestrings. But the hysterics fear failure so much that they have to abandon conventional values. They therefore pursue this make-believe success with a singleness of purpose which seems undeterred by the fact that society frowns upon admission of defeat.

5. *Normals.* Of all the groups, the normals reconcile most successfully the contradiction of optimistic estimates which still avoid failure. The cultural definition of success in terms of improved performances is reflected by the large majority of normals, who have positive median goal discrepancies. The neurasthenics and paranoid schizophrenics also conform to this cultural pressure. Unlike these groups, the normals make a serious attempt to protect themselves against failure. For more than two-thirds of the normals, the majority of their individual goal discrepancies are not positive. Yet most of them have positive median discrepancies. This points to flexibility in adjusting level of aspiration to trends of achievement scores. In other words, the normals often estimate that they will do the same or worse. But when things seem to be going well, they have the courage to risk the prediction of substantial increases. In fact, the normals are unique among the groups in their ability to reverse the direction of their predictions when such a reversal seems justified by the situation.

Compared to the psychiatric *Ss*, the normals seem less overwhelmed by the possibility of failure. Their mean reaction time, and number of *Ss* with more than two inordinately large goal discrepancies are both significantly less than those of the combined psychiatric groups.

In summary, the data tend to support the hypothesis that the five groups respond differentially to threat of failure. There remains the additional hypothesis that level of aspiration is related to such clinical characteristics

as conversion phenomena, vegetative symptoms, and delusions. This area, which was pursued through study of personal background data of the *Ss* and their reports of aspirational experience, will be treated in a subsequent paper.

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THE DISCRIMINATORY RECOGNITION OF VISUAL PATTERNS UNDER HYPNOSIS [1]

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THE majority of hypnotists not connected with the psychological or medical sciences appear to believe rather firmly in the so-called hypnotic "transcendence" of normal functions. One of their favorite, and admittedly more spectacular, demonstrations of this consists in the following: A subject is hypnotized. He is then shown the reverse side of a playing card with symmetric pattern picked at random by a spectator, and made to hallucinate something or other on its back. The card is then mixed with other similar cards and the subject requested to find it, which, if the experiment is successful, he proceeds to do as many times as desired.

When well done, this demonstration is quite convincing and impressive—so much so that Sidis (9), one of the first to mention it in the scientific literature, appeared to be convinced by it. Unfortunately, from a scientific standpoint it proves nothing, for it is clearly not demonstrated that the subject could not do the same in the waking state.

While the experiment just described has not been studied directly, a number of closely related investigations have been made. P. C. Young (15) studied subjects for discrimination between similar ink blots. He could not find any difference between hypnotic scores and waking scores. In another experiment he had the subjects look at an ordinary postage stamp, then asked them to pick it out from among other similar ones. Here, too, he reports no evidence of any improved performance in hypnosis as compared to that in the waking state. Eysenck (3) similarly tested subjects for recognition of cards bearing geometrical figures and dots, and for recognition of the reverse side of playing cards. In agreement with Young he reports no advantage of hypnosis over the waking state.

These investigations meet satisfactorily the criteria of a good experiment, but it is still not possible to draw from these results as general a conclusion as one would wish, for it is clear that not all possible conditions are represented here. For instance, the subjects were urged to do their best in both waking and hypnotic states, but they were not given suggestions that their capacity for visual perception or their memory would be improved. It remains to be shown that this does not produce any difference in performance. Then too, it may well be that the trance state, although reported as deep, may still have been inadequate to bring about detectable differences in the above instances. It is becoming increasingly evident that the mere induction of a trance and even the giving of special suggestion is not all there is to obtaining special effects in hypnosis. Allowing time for the formation of an appropriate "mental set," and for reorientation of the subject in the hypnotic situation is an important factor, as shown by Erickson (2) and others. Similarly, it may be necessary to distinguish here between the performance of passive, nonparticipating subjects, and those who are actively participating in an extensive manner in the hypnotic situation. The existence of these two types of trance subjects has been pointed out by White (12) and by Eysenck (4) among others. Finally, the question of task difficulty does not appear to have been treated adequately by past investigators. Eysenck (3) does recognize in other connections that too difficult a task would tend to prevent the appearance of any improvement in hypnosis. There is, however, no evidence to indicate this was taken into consideration in the test already cited. In connection with his stamp experiment, Young (15) reports having gone to some trouble in designing a

[1] Paper read before the Society for Experimental and Clinical Hypnosis, May, 1950.

[2] The experimental work reported here was carried out with minor exceptions during the investigator's stay at the University of Oklahoma.

test which would task the subjects to a certain extent, but he does not appear to be aware that too difficult a task might also interfere with the appearance of improved performances. Finally, in all problems of this kind the question of individual differences always arises. It is clear that a test which is difficult for one individual may be easy for another, and in between for a third. If test difficulty is an important factor in determining the final results, it becomes essential to ascertain the former for each individual subject. Neither Eysenck nor Young has done this.

The present investigation aims at repeating these studies, taking into account some of the above points. Another purpose, should it be found that hypnosis can bring about a higher level of performance, is to attempt the determination of some of the characteristics of the process involved.

THE REALITY OF SUPERIOR HYPNOTIC PERFORMANCE IN RELATION TO VISUAL PERCEPTION

The Problem Restated

Given a normal individual capable of a certain actual *maximal* performance level in respect to the discriminatory recognition of given visual stimuli while in the waking state, is it possible, other things being equal, to raise his performance level by means of hypnosis?

Procedure

Test. In order to answer this question, a test was designed combining features from both Young's stamp and Eysenck's card experiments. In essence the test consisted in having the subject look at the reverse side of a card for a predetermined interval of time, then, if possible, pick it out from among a total of ten cards presented in random order. The probability of getting the correct card by pure chance is .1. Thus, with ten repetitions of the above, the subject would be expected to choose the card correctly only once by chance. It is clear, too, that the chance of doing the same twice, three, or more times decreases exponentially in a rapid manner.

Young (15) had found in his investigation, when using stamps in a similar manner, that it was necessary to hide the borders of the stamps by means of a suitable frame, for otherwise the subjects had little difficulty in identifying the chosen stamp from among many similar ones. Keeping this in mind, and using playing cards of high quality (Bicycle Brand: Doily and Mosaic patterns), we made some preliminary investigations which showed that a similar situation existed here. The use of a special cardboard frame hiding the border of each card was found quite essential in order to obtain a test of sufficient difficulty. One of the problems which presented itself was how to present the card in the frame. Even with constant lighting and fixed apparatus, it is difficult to present such cards to *Ss* of varying stature and eye-strength without incurring the risk of all sorts of distorting effects. In the end, after further preliminary work, it was deemed satisfactory to allow *S* to hold the frame in his hands and to choose the position he preferred, with the proviso that the card and frame were to be held upright as given and could not be rotated in their plane. Close watch was kept of the *Ss*. Of 23, none attempted to rotate the frame. In fact, in most instances, the *Ss* limited themselves to adjusting the distance of the card from their eyes. Furthermore, it was found that the use of the frame was unnecessary when *S* was trying to pick out the card. This fortunate finding simplified procedures considerably.

In order to establish *S*'s limitations in regard to his ability to discriminate between similar patterns and to recognize one previously seen, a somewhat rough but nevertheless usable scale was designed. The reverse sides of a large number of similar playing cards were examined and ranked according to the order of difficulty met in detecting differences between them. Ten series of ten cards each were thus made, each series representing a different degree of difficulty.[8] Of these series, three were made up with cards some of which had been purposefully marked in order to extend the series in the range of lesser difficulty. The series were initially constructed by *E*. As a further check, they were submitted to a student who had demonstrated a superior acuity of vision in a previous test.

In this manner, a point scale of difficulty was established by rank-ordering with integral values running from 0 to 10, inclusive. The zero value was assigned to the least difficult series, and the 10 to the most difficult. In addition, two further scores, NL_s (no superior limit) and NL_i (no inferior limit), were added to the scale to cover cases which passed the most difficult series, and those which failed the easiest one, respectively.

Subjects. The *Ss* employed were all undergraduate students at the University of Oklahoma. The majority were sophomores taking their first course in psychology. They had little or no knowledge of hypnotic phenomena. All volunteered as *Ss*. After elimination of a number of applicants for a variety of reasons, 23 were retained. Of these, 2 turned out to be completely resistant to hypnosis (score of 0 on the David-Husband [1] scale), 14 ranged from deep hypnoidal (score of 15) to medium-light trance (score of 28), and, finally, 6 *Ss* were found ranging from medium trance (score of 59) to extremely deep trance (maximal score of 341).

[8] That is, all the cards within a series were of equal difficulty, but the level of difficulty varied from series to series.

The schedule of experimentation called for five sessions averaging one hour each. The six last-mentioned *Ss* and one medium-light trance *S* went through the entire schedule. The remaining *Ss* were taken only through the first session. The schedule was as follows:

Session 1
a) Subject's limit for card recognition established.
b) Suggestibility tested and hypnosis induced, if possible.

Session 2
Trance deepened. Trance depth tested.

Session 3
Optional. Used only for cases in which it was believed trance might be deepened further with more training.

Session 4
a) Subject tested for card recognition in the waking state.
b) Subject tested for card recognition in the trance state.
c) Subject tested for card recognition in the waking state.

Session 5
Repetition of Session 4.

These sessions were performed on separate days, usually with a week interval in between. All *Ss* were requested not to communicate the details of the experiment to others, nor to attempt to repeat the tests themselves. To the best of the writer's knowledge, none did.

We shall now examine these various sessions in more detail. All were held in the same room under relatively constant lighting conditions. All *Ss* were made to sit in the same position. In the first session, *S* was instructed as follows:

This experiment is a test of your ability to discriminate between very similar patterns. I have here a deck of cards, the backs of which will be shown to you. No two of these are alike. In a while, I will hand you this frame [frame indicated] in which a card from this pack will be placed. You will hold the frame in the position I hand it to you, and at a comfortable distance from your eyes. Do not rotate the frame. Look at the card and try to memorize it as best as you can in order that you may be able later on to pick it out from among the remaining cards. You will concentrate on this card until such time as I tell you to close your eyes.

After a while, I will hand you a pack of cards and then tell you to open your eyes. This pack ***may or may not*** contain the card you examined. You will go through this deck, card by card, as soon as your eyes are open, and see if you can find the card I showed you earlier. You are to go only once through the cards. If you have any doubts about a card, make a guess. Remember, the card may not be in the deck. We will repeat this a number of times.

Do you have any questions? Is this clear? [Pause.]

All right then. We will now run through these steps. Now close your eyes, and do not open them until I tell you to. Hold out your right hand. Here is the frame. Open your eyes and take a good look at the card. Try your best to memorize what it looks like. [Pause while *S* allowed to look at the card for 55 seconds.] Close your eyes now and let me have the frame. Keep your eyes closed until I tell you otherwise.

The card was then taken out of the frame and mixed randomly with the remaining nine cards of the series used. To insure perfect random mixing, each card was assigned a number from 1 to 10 (placed on the face side), and the order for each run was determined by a table of random numbers. As the cards were handed to *S*, the following was said:

Keep your eyes closed. Here is a pack of cards in which the card you have just examined may be. In a moment, I will tell you to open your eyes. As soon as you do so, please go through the pack carefully, card by card, and see if you can find the card. Do not spend too much time on any one card. In case of doubt, make a guess. Remember, the card may not be in the deck. You will be allowed a maximum amount of time with each card [ten seconds were allowed] and I will warn you when the time is up by saying: "Next." If you come to a decision before the signal, do not wait for it. If you find the card, hand it to me. Otherwise go on to the next. If you have not done either by the time you hear the signal, put the card you are examining down and go on to the next. Is this clear? [Pause.] All right, let us do this. Open your eyes and go ahead.

The above was done prior even to testing for suggestibility in order to eliminate any possibility of the instructions' acting as suggestions, as they might have if given after *S* had carried out some suggestions. A few more words might be added here concerning the method of establishing *S*'s rank on the scale described earlier. As already explained, a single test consisted in *S*'s looking at the reverse side of a card taken at random from a pack to which had been assigned a certain degree of difficulty. The card was then placed at random in the pack, and *S* was requested to see if he could find it.[4] The *S* had three types of responses available to him. He could pick the correct card (positive response), he could pick a wrong card (negative response), or, finally, he could go through the pack without recognizing any card as being the one seen (neutral response). The aim of the first session was to establish what the writer has designated as the *upper limit for discriminatory recognition* of the *S*.[5] Briefly, this was defined as being either the least difficult series for which the subject gave a random-like response, or the least difficult series for which he gave no positive responses in ten trials. (That is, the next one below this rank would get better than chance recognition.) This second criterion was necessitated because some *Ss* never exhibited chance success in the waking state, but passed without transition from better than chance to no success at all (all responses negative or neutral). Whatever the series, its rank on the scale became *S*'s "difficulty score."[6] Exceptions were the infrequent instances in which the least

[4] This was done with as many series as necessary to establish *S*'s rank. In each case, the card to be picked out was taken from the particular series being used.

[5] Hereafter, simply referred to as the "*S*'s limit."

[6] Every *S* was not tested with all ten series of cards, but, except for those falling at the extreme of the difficulty scale, each *S* was tested with at least the next two more difficult series, and the next two less difficult ones, as compared to the series establishing his limit.

difficult series was too difficult for *S*, and the most difficult, not sufficiently so. Such subjects were scored NL_1 and NL_2 respectively.

With regard to the induction of hypnosis, the standard method of visual fixation and suggestions of sleep was used. Every effort was made to induce as deep a trance as possible. Following eye closure, ten minutes were allowed for deepening of the trance. Whenever hypnosis was induced, suggestions of posthypnotic amnesia were invariably given prior to waking *S*. The suggestions and instructions used both in the control and the experimental sessions were designed with great care to exclude possible cues and the misconstruing of what was desired. Insofar as possible, they were made alike.

The instructions under hypnosis differed from the waking ones by the addition of suggestions of improved memory and perception. The instructions began exactly like those of Session 1*a* and proceeded through the first three paragraphs. This was succeeded by the following:

> Now close your eyes, and hold out your right hand. Here is the frame. Open your eyes and look well at the card. The card at which you are looking is quite distinct from other cards I may show you. There are many things about it which are different. You will have no difficulty later in recognizing it from among other cards because it will stand out quite sharply among these, being the only one with the pattern you see. You will have a very clear and sharp memory of this card and you will be able to pick it out any time. As you look at this card, you find its features stand out very sharply, and you will have no difficulty whatsoever in the future in locating it. Although very similar to the other cards, it possesses characteristics not to be found in the others. These characteristics are quite sharp, and they will make the card stand out so that you will recognize it immediately when you see it. You will have no difficulty in doing this. Now close your eyes and let me have the frame.

The cards were then mixed randomly and handed to the subject at the same time the following instructions were given:

> Keep your eyes closed. Here is a pack of cards in which the card you have just examined may be. In a moment I will tell you to open your eyes. You will remain deep asleep, but will be able to see clearly the cards. As soon as you open your eyes, please go through the pack carefully, card by card, and see if you can find this card. If it is present, you will recognize it at once, for it is the only one of its kind. It is very distinct and will stand out among the other cards. There is no possibility for you to make mistakes, so distinctive is the card. You will have absolutely no trouble in finding it. It will stand out. Open your eyes and go ahead.

The test series used for Sessions 4 and 5 were chosen so as to be of the same difficulty as the *S*'s limit.

In the final waking test, the first set of instructions was essentially employed again, minus the first three introductory paragraphs.

Results

As previously indicated, of 23 *S*s only 6 were capable of developing trance states of any sort. These 6 and 1 medium-light trance *S* were given the complete set of tests. The remaining 15 *S*s were only given the first waking test of Session 1. As the actual depth and type of trance of each *S* may be significant, they are given in Table 1. The scores are calculated on the Davis-Husband Scale. The types are according to White (12) and Eysenck (3).

TABLE 1

CHARACTERISTICS OF *S*s COMPLETING THE EXPERIMENT

N	TRANCE DEPTH	SCORE	TYPE
1	Medium-light	28	Moderately active
1	Medium	59	Moderately active
1	Deep	112	Passive
3	Deep	138	Moderately active
1	Very deep	341	Very active

The scores for discriminatory recognition in Session 1*a* are rather edifying. Of the 23 *S*s who were tested, 3 received scores of NL_4 and 3 more scored NL_3. That is, 3 passed the most difficult series, while 3 others could not pass even the easiest series. In the latter category, 1 *S* consistently gave a neutral response for all runs. The 17 remaining *S*s showed definite upper limits. Four of these had limits near the bottom of the scale, while the remainder averaged toward the middle. For the entire group of 23 *S*s, a rough mean difficulty score was 4.5. This is qualified as "rough" because the calculation assumed equal intervals of difficulty, and this is probably not the case. The range of variations in scores was from 0 to 10 (excluding the 6 cases without limits). For the trance group alone, the mean performance was 6.0, with 1 *S* giving a score of NL_4.

In regard to the hypnotic scores, all but one *S* gave nearly identical responses in the pretrance, trance, and posttrance tests, thus showing no advantage for hypnosis over the waking state. As a matter of fact, in a number of instances waking scores were slightly superior to the hypnotic scores. This, however, cannot be shown to be statistically significant. On the other hand, one *S* did show a remarkable superiority of performance in hypnosis. This *S*, to whom we shall hereafter refer as L. G., gave a perfect score on the Davis-Husband Scale. His limit for dis-

criminatory recognition was established at 7. In the waking state he consistently gave random scores on the card test, while in the trance state he showed 100 per cent recognition, picking the correct card without hesitation within two seconds in each run. Since the probability of picking a card correctly ten times in succession in ten runs is 10^{-10}, this difference in performance is highly significant.

Because of this outstanding performance, L. G. was singled out and used for further investigation of his ability for discriminatory recognition of cards. Unfortunately, lack of time did not allow as extensive an investigation as would be desirable, and the material to follow can only be considered of a preliminary nature.

FURTHER STUDY OF SUBJECT L. G.

Before describing further work done with L. G., some observations made by the investigator on another *S* a few years ago will be described briefly.

The *S*, whom we shall call Ella, was a woman in her thirties who had proven to be an excellent *S*, probably of hypnotic depth comparable to L. G.'s, although of a more passive type. At the time the writer made her acquaintance, she was being used by a professional hypnotist for purposes of demonstrations. One of the experiments this *S* often performed was the card experiment already described. This she would invariably perform with 100 per cent success, usually picking out the card from among a regular pack of 52. This ability was exhibited both in the waking as well as in the posthypnotic state. Whether or not Ella could have done the same in the absence of hypnotic suggestions was never investigated. In view of the fact that this *S* had performed the experiment innumerable times both while in the trance and posthypnotically, and, furthermore, was well trained in the production and use of self-hypnosis, it is rather questionable whether simple control tests would have led to anything conclusive.

On one occasion, it was observed that sometimes, prior to announcing the correct card as she went through the pack, Ella would rotate it 180° in its plane. Upon inquiry, she explained that the card was "upside down." To investigate this new response, a number of runs were made in which the chosen card was deliberately inverted or left in the same orientation as when she had first seen it. This was done with a number of cards and packs taken at random. Without fail, the *S* was able to detect the inversion, always rotating the card before declaring that it was the card previously seen.

It may be noted here that in all these runs, Ella was made to hallucinate a picture on the back of the card, and, in talking, she always referred to the picture and not to the card. In order to gain a little more insight into the processes involved in this type of discriminatory recognition, it was suggested she would see a uniform circle the size of a quarter with the center coinciding with that of the card. After ascertaining that she did, the above tests were repeated with the card inverted from time to time. The behavior was observed to be the same as before, the *S* again reporting on inquiry that she turned the card because "the circle was upside down." This is a rather remarkable statement, if the circle was truly seen as a symmetrical figure centered in the card!

About this time the writer had to move to a different locality, and further work with Ella had to be discontinued. These results, however, appeared to indicate that there might be more to the problem than had been at first anticipated. But it was also clear that it was essential to determine whether there could really be superior performance in hypnosis. Thus the present investigation was born.

L. G.'s ability to recognize a card among 10 others was tested further. He was required to pick the card from among 52. This, too, was done without difficulty. As a further test, he was asked to look at 3 cards which were then mixed with 49 similar ones. This particular test did not appear to offer appreciably greater difficulty to L. G. when he was in a trance. Similar tests tried later in the posthypnotic state had similar success. A further experiment was then devised. Five blank, white, 3 by 5 index cards were used. These were picked out with special attention to uniformity and absence of blemishes. L. G. was then tested in the waking state to ascertain whether he could recog-

nize any of these cards if first seen alone, then mixed with the other four. No better than chance responses were observed. Following this, L. G. was hynotized as usual. One of the cards was then shown to him, and it was suggested that it was a brilliant blue. After ascertaining that he did see it as this color, and after telling him to keep seeing it thus, it was mixed with the remaining cards. Suggestions were given that he would be able to pick out the card because of its distinctive color. Then the cards were given to him. The "blue" card was picked out in this manner a number of times without hesitation.

To make the test more difficult, the next step involved presenting three of the five cards and having L. G. hallucinate these as brilliant blue, brilliant green, and brilliant red, respectively. These were then mixed with the other two and a series of presentations made of these cards, one by one, with the request that he call out their colors. Originally L. G. was shown separately each card for which a color was hallucinated. On testing for recognition, a certain amount of confusion appeared to be present. He was able to pick out the cards which had colors associated with them, but tended to confuse two of their colors. Even after the first step of the test was repeated with the same cards this tendency toward confusion persisted. To remedy this, and on the assumption that it was caused by the confused cards' being too much alike, the three cards were presented together and each color called out. After the *S* had been made to run through the colors a few times while the cards were visible to him, these were once more mixed, and the experiment proceeded as before. This time no further difficulties were met. This test was also repeated posthypnotically and gave similar results when done shortly after waking, but *S* showed confusion if longer intervals of time were allowed to elapse. This last has been reported elsewhere (11). Finally, L. G. was tested with series of greater difficulty than the one used in the main experiment. He still could pick out the cards correctly in every trial, but showed a noticeable slowing of response.

In order to find out more concerning the processes involved in this ability, the effect of distance was investigated. It may be noted here that L. G. had better than average visual acuity. At no time were the distances involved such as to task his eyes. As usual, L. G. was allowed to examine the chosen card for the usual prescribed interval of time, but now the card was held by *E* and its distance from *S* was recorded on a yardstick. This position will be called hereafter the *initial position*. In the recognition test, the cards used were presented at various distances, and L. G. was requested as usual to state whether it was the card previously seen. Regardless of his answer he was told to watch closely while the card was displaced horizontally either toward or away from him according to its initial position.[7] He was requested to signal should the card seem definitely different at any instant. The only information he was given concerning the experiment was that the possible effect of distance on his perception of patterns was being tested. A total of ten determinations was made, five with the card moved toward him, and five away from him. This was done with the initial position placed at 1, 1½, and 2 feet from L. G. (as measured from his chest).

The results were rather interesting, for invariably when the chosen card was placed beyond its initial position, *S* did not recognize it. But as soon as it was within a range of three centimeters on either side of the initial position, he would signal his recognition of the card. Thus, within a range of variation of six centimeters, it was possible to determine the initial position of the card by *S*'s sudden recognition of it. Later questioning showed that he was not aware of the true nature of the experiment. While, admittedly, this particular experiment was done somewhat crudely, these results appear significant to the writer. It is true no head rest or other similar gear was employed and, as a consequence, the *S* may have moved some. No motion of appreciable extent was noted, however. Similarly, the illumination of the card may have varied some with change in position. But the *S*'s answers when questioned concerning this factor lead the investigator to believe this was not a crucial factor in the present study.

[7] That is, if the card was between him and the initial position, it was moved away from him, and conversely.

DISCUSSION

Within the scope of this experiment, and subject to its limitations, the results obtained would seem to indicate the possibility of a genuine increase in visual discriminatory recognition in hypnosis over that in the waking state. It is, however, to be noted that a number of individuals were found who gave in the waking state a performance as good, if not superior, to the best of the ones given by hypnotized *Ss*, even though some of the latter, such as L. G., gave performances very much superior to their own waking efforts. The results of this investigation also show that there is considerable individual variation in the ability to recognize a card mixed with similar cards after seeing it once. This is equally true for *Ss* of a low degree of hypnotizability and those who show considerable trance depth. The results would also appear to indicate that whatever improvement can be thus obtained, it is not without limits. The fact that L. G. could still give a perfect performance with the most difficult series but showed a slower response, tends to support this. This, incidentally, is also in agreement with a remark made by the writer in a previous communication (11), that hypnotic performance is a function of the task difficulty. Had the difficulty scale been extended further, it might well have been found that L. G.'s hypnotic performance could not come up to the best observed waking performance of other *Ss*.

This brings up a remark which Young (14) has made. This author, on the basis of his negative findings, offers as an explanation of previous reports of superior hypnotic performance the possibility that the *Ss* involved may have had an abnormally low capacity for discriminatory recognition. It is quite clear from the present study that if one picks the norm to be around 4.5 on the present scale of difficulty, some individuals do fall below the normal. In fact, of 23 *Ss*, 9 obtained scores of less than 4.5. Thus there is some justification for Young's remark. But, interestingly enough, the only *S* who showed improvement in hypnosis also made an initial score of 7, and this is definitely above the norm. As a matter of fact, only 4 other individuals made superior scores. This would, therefore, appear to invalidate Young's hypothesis. Furthermore, it seems to the writer that the point here is not whether individuals can be made to transcend a group norm, but rather, whether they can be made to perform *above their own individual norm*. The position taken here is that it is immaterial whether the *S*'s performance is normal or low, as long as it can be shown that his best *voluntary* performance in the waking state is below that in the hypnotized state, with or without special suggestions and procedures.

Leaving this matter, one may now consider possible reasons for the disagreement between the present results and those reported by Young and by Eysenck. Some of these were pointed out earlier. In the first place, in the main experiment, special suggestions were given in all instances to the effect that the *Ss* would be capable of passing the test. Whether this really made a difference is hard to tell since later tests made with L. G. showed that the suggestions could be reduced to just the statement: "You will be able to pick out this card anytime you see it." On the other hand, the writer has long been of the opinion that after repeated use of a suggestion with a good *S*, one needs only to use a few cue words from it to obtain the same results.

Another point of difference might be that the difficulty of the test used was fitted to the *S*'s characteristic capacity. Two facts argue against this being the main factor here. First, L. G. was later shown capable of passing the more difficult test series when hypnotized. Secondly, none of the other trance *Ss* appears to have been helped by this particular item.

There then remain trance depth and the type of hypnotic *S*.[8] These, the writer is inclined to believe, are the main factors. The difference in trance depth between L. G. and the other *Ss* is very large. In fact, there is really no comparison. Again, L. G. was the only *S* showing a truly active participation in the hypnotic situation. Finally, he received far more hypnotic training than any of the other *Ss* and had participated in numerous

[8] The possibility of small, involuntary cues from *E* seems rather well taken care of by the fact that *E* did not know at any time which card was being examined, nor what the chosen card looked like. Except in the early investigations made with Ella, great care was taken in this respect.

sessions prior to participating in the present investigation. Unquestionably, until more *Ss* of each kind can be obtained and tested as in the present investigation, the reasons for L. G.'s success must remain primarily suppositions.

Turning our attention toward other matters, we may now consider briefly the nature of the process involved. Initially, the investigator had conceived of this experiment as one dealing with visual perception. On the other hand, Young and Eysenck appear to think of it as primarily concerned with memory. On further analysis, it would appear to partake of both. A double process seems to be involved: (1) The *S* must be able to perceive either details or a pattern which characterizes the card well enough to allow later discrimination. (2) He must be able to carry in his "memory" an accurate enough replica of the salient points perceived to permit later recognition and differentiation. It may well be that the discriminatory process is itself a third component.

Disregarding this last possibility, it is clear that failure either to perceive salient features or to carry an accurate "memory" of these will tend to prevent *S* from recognizing the card when he sees it again. Which factor is the more important is not shown by the present study. Neither can we tell whether only one or both of the processes are affected. In any case, it must be clearly understood that any reference made in this report to "perceptual" capacity, acuity, and so on, must of necessity include both factors and denote a composite phenomenon or process. One small clue does exist concerning the factor predominantly affected. Previous studies of the effects of hypnosis on memory (10, 6, 7, 14, 3; and, particularly, 13, 8) have indicated that hypnosis favors learning and recall in proportion to the degree of meaningfulness of the memorized material, no improvement being found for meaningless material or material showing little organization and integration in a context. Patterns such as are to be found on the backs of cards, as well as blank cards, seem to fall rather well within the category of "meaningless material." If this be true, hypnosis would not be expected to affect the memory factor in the above scheme, and, hence, any observed improvement would be due primarily to an actual improvement in perceptual functions. Furthermore, if some *Ss* have a very poor memory for the sort of features to be found on the backs of the cards, hypnotic improvement of their perceptual process might still not be able to overcome their memory weaknesses, and, hence, no improvement could be detected.

Does *S* perceive a pattern as a whole or only details? This is an important question which *E* had hoped to answer, but could not do because of various circumstances. The following, however, seems to be true. All *Ss* who scored NL_0, that is, passed the most difficult series in the waking test (Session 1), were found upon inquiry to have singled out specific details in the form of minor defects in the cards. These in some cases were amazingly small. One *S*, for instance, reported a small brown spot invisible to the investigator. Examination with a low-powered magnifier revealed a minute red thread at the indicated spot.

The remaining *Ss* apparently did not seek particular details. This seems to be indicated by their verbal reports at the end of the experiment. In a few cases, in fact, the *Ss* spontaneously stated during the sessions that they did not know "what to look for." Subject L. G. was repeatedly questioned concerning the cues which enabled him to recognize the cards. He reported that no specific single detail or group of details was ever involved. At least, he appeared never to have been aware of the existence of such features in the cards submitted. According to him, the card either "looked" or "did not look" like the original one. He could never point to any specific distinguishing features. This persisted even after he was allowed at a later date to witness in the waking state a performance in which an *S* made use of card defects as cues and pointed out each of these at *E*'s request. L. G. reported seeing them at the time. Insofar as L. G. was concerned, it would seem that he perceived a whole pattern, that is, a Gestalt. Further support for this comes from the experiment described earlier in which the distances of the cards were varied. This also seems to be indicated by those experiments in which the cards were rotated 180° before presentation. If *S*'s recognition depended only upon some specific

blemish or blemishes having a certain position in space, such a rotation would tend to confuse him. But neither L. G. nor Ella ever hesitated in detecting the cards, whether they were rotated or not. Why Ella always turned the card to its original orientation before calling it is not clear. Since she never did this with the other cards, it was not due to just a guess based on partial recognition. The author had the distinct impression that Ella fully recognized the card before she turned it around. It is an attribute of Gestalts that they preserve much of their quality under various manipulations. Thus the letter *E* is perceived as such whether it be inverted, or shown in some other arbitrary orientation. A pattern seen as a whole would also have this sort of invariance with respect to rotations and would account for the above behavior.

As a final note, it may be remarked that the above far from exhausts the possibilities for investigation. For instance, the factor of time has not been considered at all, beyond employing fixed time intervals throughout. Again, a more extensive scale of difficulty might enable one to detect smaller increments in discriminatory recognition than could be detected here. The nature of the stimulus itself bears further investigation. It is the intention of the *E* to supplement this report in the future by following up some of these lines of approach, and from this standpoint the present material can only be considered as a preliminary investigation.

SUMMARY AND CONCLUSIONS

1. The *E* has investigated the effects of suggesting under hypnosis a heightening of the *S*'s ability to recognize a card from among similar ones, after having seen the card once, briefly, prior to the recognition test.

2. Test material was constructed in such a manner as to establish a rough scale of difficulty. A study of 23 *S*s shows that the ability to recognize a card in the above manner varies with individuals as well as with the difficulty of the test material. The majority of individuals appear to fail with material of average difficulty (on the present ten-point scale), with a few passing all items and a few passing no item.

3. Under hypnosis, all but one *S* failed to show performance superior to that exhibited in the waking state. These five *S*s varied in trance depth from light to deep, and were either of passive type or mildly active. The sixth subject, L. G., exhibiting trance of great depth, was of the active type, and gave a perfect performance under hypnosis as against a chance performance in the waking state.

4. Tests in which the card was presented to L. G. under hypnosis at varying distances showed that his recognition of it depended considerably upon the card's having the same position as it had when first seen. However, when the card was rotated 180° in its plane, this change did not interfere, although it was detected.

5. L. G. was also able to pick out white blank index cards which had been hallucinated as colored, when these were mixed with similar cards.

6. L. G.'s responses were found to be slower when test difficulty was increased.

7. Discriminatory recognition of the kind discussed here would appear to involve two processes: (*a*) the perception of specific characteristics of the cards; and (*b*) fixation of the percept for recall. Hypnosis may affect either or both. On the basis of older investigations it is probable that perception is most affected.

8. It is concluded that insofar as card recognition is concerned, and under the limitations imposed by the experimental conditions, hypnotic suggestions of improved differential recognition can bring about a level of performance superior to that of the waking state. This is, however, not true for all individuals. Presumably, only those individuals who are not functioning at their maximum capacity in the waking state are capable of exhibiting improvement under hypnosis.

9. Failure to show improvement when maximal performance is not given in the waking state may result from: (*a*) failure to obtain a deep enough trance; (*b*) insufficient active participation of the *S* in the hypnotic situation; or (*c*) inability to fixate the percept for recall (poor recall).

10. Subjects appear to fall into two groups, those who pick out single details in the cards, and those who apparently "memorize" the card as a whole. Those *S*s who give per-

fect scores in the waking state appear to be the ones who make use of details.[9]

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GROUP STRUCTURE AND PERCEPTION[1]

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IN his study of social factors in perception, Sherif (10) showed that verbally expressed judgments of individuals as to extent of autokinetic movement, initially widely divergent, converged on a common norm in a social situation. The same convergence after group discussion for estimates of the number of beans in a jar had been previously demonstrated by Jenness (6).

In Sherif's experiment, however, it was further established that when *Ss* who had participated in the group situation were tested alone, their perception of the extent to which the light moved followed the previously established group norm. That social modification of perception would also hold for more highly structured perceptual fields was demonstrated by Schonbar (9), who had pairs of *Ss* estimate discriminably different extents of movements of a point of light. In the final alone situation, each *S* tended to see the light move a distance commensurate with the norm established in the paired situation.

Three possible factors in the determination of the extent to which the individual's perception will be modified in a group situation are (1) the structure of the perceptual field, (2) the personality of *S*, and (3) the structure of the group.

Sherif argued (12) that the degree to which the perceptual field is structured serves to limit the influence of social factors in perception. This hypothesis is supported by the experiments of Luchins (7) and others. In her previously mentioned study, however, Schonbar (9) obtained just as much convergence on a common norm in perceptual fields of high structure as in perceptual fields of medium structure. Her work certainly proved, in any case, that the principle of social modification of perception could be extended from ambiguous perceptual stimuli to concrete, objective stimuli.

A study by the writer (2) indicated that in the same experimental situation, *Ss* show striking differences in their susceptibility to social norms for the perception of autokinetic movement. It was suggested that these differences are related to personality characteristics of the individuals concerned.

The work of Slavson with group therapy (13, 14) and the findings of Grinker and Spiegel (5) in regard to the influence of the "military super-ego" on combat personnel, suggest that the degree to which social norms are interiorized in a group situation, and, therefore, the extent to which they can modify the individual's perception, depends upon the individual's relationship to the group—which is in turn a function of group structure.

Two kinds of group can be postulated to represent the extremes of such relationship. In the therapy group of Slavson, the individual through interaction and cooperative effort could scarcely avoid being drawn into relationships with others. In a group like the traditional academic class, the individual's social contacts with others in the group would be severely limited, and he would be relatively isolated. He would have little opportunity for interaction.

THE PROBLEM

The present study was launched on the supposition that the degree of conformity to a social norm obtained within a group is a function, not only of the personalities of group members and of the structure of the perceptual field, but also of the way in which relationships within the group are structured. The recitation sections from two one-semester psychology courses offered in the winter, 1947–1948, semester at the University of Michigan embodied, respectively, two contrasting group structures which would, it was felt, provide an experimental test of this supposition.

In the course in interpersonal relations, specific techniques such as the gradual retirement of the teacher from discussions, seating of students in a

[1] This study is part of a doctoral dissertation completed at the University of Michigan in 1949 under the general direction of Dr. George Satter, Dr. Harold Guetzkow, and Dr. Donald G. Marquis, to whom the author is indebted for their encouragement and expert guidance.

circle, and use of small sections (those sections selected for the experiment averaged 20 persons) had the effect of maximizing verbal interaction among group members. During the course of the semester, the role of the section leader became, according to plan, more and more that of member of the group, and group decisions on various matters pertaining to the group and the course were encouraged. These procedures were standardized through daily luncheon sessions of the staff presenting the course.

A more traditional academic pattern was followed in the course in elementary psychology, where students sat in rows, classes were comparatively large (those used in the experiment averaged 30 persons), and where, while the leader participated in an encouraged discussion, the verbal interaction (in part owing to the physical arrangements) was channelled largely between teacher and student, rather than among students as in the recitation sections of the other course.

The role of the leader was here separate and distinct from that of the role of group member, and while the leader deferred to the expressed opinions of individuals in the group on various matters, no specific group decisions were undertaken as a rule. Procedures were more or less standardized through weekly conferences of the teaching staff for this course, and while there was not a great deal of variation from the leader-centered pattern, this standardization was more the result of circumstance and tradition than deliberate design on the part of the staff.

A supportive leader-member relationship obtained with minor variations throughout both courses. The recitation sections of both met for a total of two hours each week, and each course lasted for one semester.

The essential differences between the two kinds of group structure were in respect to (1) amount of verbal interaction among group members, (2) role of the leader, and (3) the occasional use of group decision. The big day-to-day procedural difference between them was undoubtedly the amount of verbal interaction among members. In the interpersonal relations course sections, such interaction was fostered by referral of teacher-directed questions back to the class whenever possible, encouragement of cross talk and debate among students, and the seating arrangement. In the sections of the elementary course, following the usual academic pattern, the verbal interaction was largely concentrated between teacher and student rather than between students. Results from a recent study (3) indicate that the major procedural difference between the two kinds of group is the verbal interaction among members.[2]

Hereinafter for the present study, the interpersonal relations course sections will be referred to as *group-centered*, while the elementary course sections will be referred to as *leader-centered*.

The specific hypothesis to be tested is, therefore, that more convergence on a common norm for perception of an objective stimulus will be obtained following presentation of individual judgments and their average for the stimulus, in group-centered structure than in leader-centered structure.

[2] See footnote 5, below.

METHOD

Apparatus

The stimulus to be used had to be one that as far as possible eliminated the factors of past experience and familiarity, yet it also had to be one that was part of a concrete, objective situation. The stimulus chosen after initial experimentation was simply a green rectangle of paper pasted on a larger rectangle of greyish-white cardboard. The length of the green rectangle was the stimulus to be judged.

Administrative Procedure

As in the Sherif (10) experiment, it was essential in this study to make certain that *Ss* remained naive as to the real purpose of the experiment. On being introduced by the group leader, therefore, the *E* requested cooperation of the group in a brief experiment in perception. He asked group members to estimate as accurately as possible the length of a green rectangle to be shown them. Judgments were to be made in inches and written on small slips of paper with no names or identifying marks attached. The *Ss* were told they would have to estimate length of the green rectangle again in four minutes, to determine what effect the passage of time would have on the accuracy of their perception. The *E* then exposed the rectangle in a convenient position for 30 seconds.

The slips of paper containing the first estimates were collected, and so that the group could hear each judgment, they were read out loud to the *E* by an *S* selected at random. The *E* then calculated the mean of the judgments and announced it clearly to the group, but in a casual manner that indicated this part of the experiment was a by-product.

After glancing at his watch, the *E* then announced, regardless of the precise amount of time elapsed, that the four minutes were "just about up" and the green rectangle was exposed in exactly the same position as before for another 30 seconds, *E* remarking, "Judge as accurately as possible, please."

The difference in standard deviation between the two sets of judgments was then obtained, providing a measure of how much convergence had followed presentation of the group average.

Selection of Subjects

Since a previous study by the writer (2) suggested that individuals vary greatly in their susceptibility to social norms for perception, it became a matter of some importance in the present experiment to discover whether any selective factors had been at work that might have tended to concentrate the more susceptible *Ss* in one course rather than in the other. For example, there was a greater proportion of girls in the interpersonal relations course

than in the elementary course. If, however, no such selective factors were at work in regard to susceptibility to social norms, then, it was hypothesized, there should be no difference in amount of convergence on a common norm between sample sections from both courses on the *first day of a semester*.

This hypothesis of no difference was put to the test on the first day of the second semester of the 1947–1948 academic year, using 129 elementary psychology students in four sections and 137 interpersonal relationship students in six sections. Choice of section was arbitrary and was dictated both by convenience in scheduling and by an effort to keep the size of the sample from each course approximately equal. The total samples used of the elementary course and the interpersonal relations course amounted to approximately 10 per cent and 17 per cent of the total course population, respectively.

An examination of Table 1 shows that the convergence of both types of group structure towards a common norm is statistically significant. The question is, however, whether the *difference* in amounts of convergence between groups of students selected from the two courses is statistically significant. Inspection shows that the initial *SD* of the leader-centered sections diminished by 0.39 inches, and the *SD* of the group-centered units by 0.29, following presentation of the estimates and their average in each group. Is this difference in amount of convergence, 0.10 inches, statistically significant or not?

Recourse was had to a formula recently developed by McNemar to obtain the standard error of this difference.[3] The standard error obtained was 0.14. The resultant critical ratio, 0.71, was not such as to indicate that any significant difference did exist between the amounts of convergence, and the hypothesis of no difference was not disproven.

The findings suggest, but do not conclusively demonstrate, that no selective factors were operating that might have introduced a bias into the experimental results by channelling the more susceptible students into one course or the other.

The ground has now been cleared for a consideration of the major hypothesis of the present study, namely, that greater convergence on a common norm for the perception of an objective stimulus will be obtained in group-centered structure than in leader-centered structure.

Groups chosen for the test of this hypothesis were from both courses in the last weeks of the first semester in the 1947–1948 academic year. The *Ss* were a total of 119 students in four sections from the elementary course, and a total of 119 students in six sections from the course in interpersonal relations. The criteria for choice of groups were again convenience in scheduling and an effort to keep the numerical size of the sample from each course about equal. The total sample from the elementary course represented roughly 10 per cent of the course population, while the sample from the interpersonal relations course represented approximately 14 per cent of the course population.

TABLE 1

CHANGE IN STANDARD DEVIATION OF ESTIMATE OF RECTANGLE LENGTH BEFORE AND AFTER PRESENTATION OF GROUP NORM, BEGINNING OF SEMESTER

TYPE OF GROUP	*N*	*SD*(1)	*SD*(2)	DIFF.	$SE_{diff.}$	*CR*	*P*
Leader-centered	129	1.41	1.02	0.39	0.107	3.64	<.01
Group-centered	137	1.18	0.887	0.29	0.089	3.25	<.01

RESULTS

Convergence

It will be noted from Table 2 that the convergence for *Ss* in leader-centered sections was 0.188 inches, while convergence for those in group-centered sections was 0.637 inches. The difference in amount of convergence is 0.449. The standard error[4] of this difference is 0.148, yielding a critical ratio of 3.03 and a *P* of less than .01.

The obtained difference is therefore statistically significant, and is in the predicted

[3] The formula for the standard error of the difference between standard deviation differences,

$$\sigma_{(\sigma_1-\sigma_2)-(\sigma_3-\sigma_4)}=\sqrt{\sigma^2_{\sigma_1-\sigma_2}+\sigma^2_{\sigma_3-\sigma_4}}$$

is based on McNemar's general formula for the standard error of the difference between two changes (8, pp. 78–79). It should be noted here that since the estimates for rectangle length were completely anonymous, the identities of persons making them could not be ascertained, and hence the actual differences between the first and second judgments of individuals could not be used to compute $\sigma_{\sigma_1-\sigma_2}$ and $\sigma_{\sigma_3-\sigma_4}$ in the above formula. For this reason, the correlation between the first and second judgments could not be taken into account, thus making the two standard errors of difference between first and second standard deviations obtained, too large. This has the following presumptive effects: (1) the conclusion of *no difference* in convergence between samples of both populations at the beginning of a semester, as presented above, becomes less certain, while (2) the conclusion of a *significant difference* in convergence between samples of both populations at the end of another semester, presented below, becomes more certain.

[4] Again making use of McNemar's formula. See footnote 3, above.

TABLE 2

CHANGE IN STANDARD DEVIATION OF ESTIMATES OF RECTANGLE LENGTH BEFORE AND AFTER PRESENTATION OF GROUP NORM, END OF SEMESTER

TYPE OF GROUP	*N*	*SD*(1)	*SD*(2)	DIFF.	$SE_{diff.}$	*CR*	*P*
Leader-centered	119	1.07	0.882	0.188	0.09	2.1	<.05
Group-centered	119	1.58	0.943	0.637	0.118	5.39	<.01

direction. The hypothesis that presentation of individual estimates and their average for perception of an objective stimulus will modify the individual's perception of that stimulus in the direction of a common norm to a greater extent in group-centered units than in leader-centered units, has therefore been demonstrated to be tenable.

Divergence

Further examination of Table 2 will indicate an unexpected result of this experiment. The initial dispersion of rectangle length estimates, before presentation of the individual judgments and their average, was greater in the group-centered sections than in the leader-centered. Table 3 shows that this difference, obtained at the end of the semester after each unit had met for a total of approximately 30 hours, is statistically significant.

What this means is that, before they know what the group average or the judgments of others are, individuals in group-centered units are freer to make extreme estimates of rectangle length than are individuals in leader-centered groups. Once the judgments of others and the group average are presented, however, group-centered unit members make a greater change from their original estimates in the direction of a common norm than do leader-centered unit members.

This leads to the paradoxical conclusion that while group-centered units have power to induce greater conformity to a perceptual norm *once that norm is made known* than do leader-centered, at the same time group-centered units permit what might be termed greater freedom from conformity *before the group norm is made known* than do leader-centered.

These conclusions are, however, based on samples from both populations at the end of a semester. A contrary result is obtained when samples from both populations on the first day of (another) semester are compared, as is shown in Table 3. Precisely the opposite trend in divergence of initial estimates is apparent: there is significantly greater dispersion in the estimates from the population that was to undergo leader-centered procedure.

This would suggest that, going into the experiment, the elementary psychology students that were to make up the leader-centered population began with greater freedom to make extreme estimates of rectangle length than did the interpersonal relations students who were to make up the group-centered population.

The apparent result of 30 hours of leader-centered, traditional academic procedure in the elementary course was, then, to *decrease* the originally high dispersion of estimates from this population. On the other hand, the apparent result of group-centered process was to *increase* the originally low dispersion of estimates from this population, until at the end of the semester, samples from both courses showed a significantly higher dispersion for the group-centered population. This argument must, however, remain tentative, since it is based on samples of both populations from the end of one semester and from

TABLE 3

COMPARISON OF INITIAL DISPERSIONS OF PERCEPTUAL ESTIMATES

	SD(1):G-C	*SD*(1):L-C	D_{90-10}	$SE_{diff.}$	*CR*	*P*
End of semester	1.58	1.07	0.51	0.123	4.2	<.01
Beginning of semester	1.18	1.41	0.23	0.112	2.05	<.05

the beginning of another. The underlying assumption is that there was no essential difference between the population of students who took the one-semester elementary psychology course in the winter, 1947–1948, semester, and the population of students who took it in the spring, 1948, semester, and that, similarly, there was no essential difference between the populations of the interpersonal relations course for these two semesters.

Even if the above assumption can be accepted, it is still necessary to account for the lower initial dispersion of perceptual judgments found on the first day of the semester in the population that was to undergo group-centered process, students in the interpersonal relations course. It should be noted that these students were required to have previously taken the elementary psychology course (leader-centered). They had therefore necessarily experienced *more* leader-centered process (which was, after all, the typical academic technique) than those just beginning the elementary course, since they were at least one semester further along in college. And it can be suggested that this greater experience with the traditional leader-centered method is reflected in a correspondingly smaller initial dispersion of perceptual estimates.

A theoretical explanation of the contrasting effects of group- and leader-centered technique on dispersion of perceptual estimates is given below in the discussion section.

Relation between Convergence and Divergence

Since both greater initial dispersion and greater convergence are manifested in group-centered structure, the question naturally arises as to what relationship obtains between these two variables. First, it can be suggested that the greater convergence is in part a function of the greater initial divergence in group-centered units. A rank-order correlation between the size of the initial standard deviation and amount of convergence obtained on presentation of the group norm for 21 groups of both kinds yields a coefficient of .70. But if the initial dispersions of the group- and leader-centered sections tested to obtain the data for Table 2 are mathematically equated by expressing the absolute change in standard deviation for each section as a proportion of the initial standard deviation for that section, greater relative convergence is still obtained for group-centered sections. The weighted average percentage of change with respect to the initial standard deviation was 19 for the four leader-centered units tested, and 34 for the six group-centered units.

Thus, whatever factors can be found to account for the greater initial dispersion in group-centered units will not serve to explain completely the greater convergence in this type of group.

DISCUSSION

It has been suggested previously that one of the major operational differences between group- and leader-centered structure is the amount of member-to-member verbal interaction in each, with such conversation being specifically encouraged in group-centered units and gently discouraged in leader-centered ones.

In a replication of the present study, involving 82 *Ss* in two pairs of groups, with one matched group-centered unit and leader-centered unit in each pair, it was found that the major procedural difference between group- and leader-centered structure was such verbal interaction. A questionnaire showed that members of group- and leader-centered units perceived no essential difference in the role of their respective leaders; and as far as could be ascertained, the frequency of group decision was not a highly relevant variable. Observations of three-week samples of all four groups by a total of 42 observers recording one session each showed, however, a substantial difference between the two types of structure in the amount of verbal interaction among students.[5] These findings are summarized in Table 4.

[5] Incidentally, the replication confirmed the finding of the present study, making use of leader B's two groups (see Table 4), that both greater convergence and greater initial dispersion in estimates of the green rectangle's length would be obtained in a group-centered unit compared to a leader-centered unit, where the *Ss* were naive. The differences were acceptable at the 5 per cent level of confidence for convergence and at the 1 per cent level for dispersion. The *Ss* in leader A's two groups were sophisticated in regard to the background of the green rectangle experiment and no differences of any statistical significance were found in regard to either convergence or dispersion between these two units.

How can this difference in verbal interaction, as the independent variable, be related to the obtained effects in perception of rectangle length?

It will be convenient to take up first the matter of the wider initial divergence unexpectedly found to be characteristic of group-centered structure. An explanation comes to hand if the initial assumption is made, subject to experimental check, that individuals in the group-centered unit tend to make the same sort of extreme judgments of a perceptual stimulus that they might if they were alone. Individuals in the leader-centered unit, on the other hand, being more isolated from their fellows within the unit, are more subject to what can be termed the pressure for social conformity—as F. H. Allport put it, the "attitude of submission which we assume, often unconsciously, in the presence of a group" (1, p. 277).

Allport used this to account for his finding, otherwise difficult to understand, that in estimating weights and the pleasantness of odors *Ss* tended to make more extreme judgments in the alone compared to social situations. There was no communication between *Ss* in the latter.

Now it can be seen that the kind of group Allport used for his social situation greatly resembles the leader-centered units of the present experiment, in that verbal interaction among members was severely curtailed. Therefore it seems reasonable to argue that the same sort of social conformity pressure that operated in Allport's experiment to dampen the extremes of his *Ss*' judgments of weights and odors, in a social situation, operated in the leader-centered groups of the present experiment to dampen the *Ss*' initial estimates of rectangle length.

This pressure can be defined as the individual's interiorized anticipation of disapproval from the group for some specific action on his part, and such anxiety could conceivably act to restrict the general movements of the body—for example, gestures might tend to be abortive rather than broad and sweeping in this type of group—in the manner described by Soskin (4). Such anxiety could also conceivably restrict the extremes of judgment for a perceptual stimulus, although the mechanism by which this could be accomplished is not obvious. But if this hypothesis can be accepted, a complete explanation of the results obtained requires the further hypothesis that the leader-centered member will experience more anxiety of this type than the group-centered member. Why should he?

It can be suggested that the individual in this kind of group, because of his lack of verbal interaction with the others in the group, is simply not aware of that area of behavior in which the group will demand or expect conformity from him. In the group-centered unit, the individual has a much clearer concept of what might be termed the group's *conformity zone*. It is clear to a much greater extent in this type of unit what sort of behavior will or will not be approved. The boundaries of the conformity zone are much more plainly marked.

Knowing what he can and cannot get away with, then, the man in the group-centered unit is freer to make wild guesses or extreme estimates on even an objective stimulus like the green rectangle. The social conformity pressure in this kind of group is strictly confined to the zone of conformity and does not pertain to behavior outside this range. But the man in the leader-centered group does not know where the limits of the conformity zone are, or what is expected of him. The pressure for social conformity therefore operates over a much wider range of behavior in this type of group, and is not,

TABLE 4

STUDENT-TO-STUDENT REMARKS EXPRESSED AS A PERCENTAGE OF TOTAL STUDENT REMARKS

LEADER	GROUP-CENTERED		LEADER-CENTERED		$SE_{diff.}$	CR	P
	N	%	N	%			
A	533	61	93	10	0.019	26.8	$<.01$
B	266	34	7	2	0.018	17.8	$<.01$

so to speak, reality-based. The individual manifests this restraint in restriction of movement and judgment.

Hypotheses have been presented to account for the greater initial dispersion of estimates of rectangle length obtained in the group-centered unit, and to the extent that they account for this phenomenon, they also can be held partly accountable for the related phenomenon of greater convergence. But not entirely, since the rank-order correlation of .70 obtained between divergence and convergence only disposes of about half the variance in the latter variable.

It is necessary to account for the finding that when the factor of initial dispersion is held constant mathematically, greater convergence is nevertheless still manifested in group-centered units.

Further experimentation with group- and leader-centered units (3) suggests the simple hypothesis that the individual's conformity to a perceptual norm is a function of how much he likes the group. One consequence of the maximized verbal interaction in the group-centered unit was that the members liked their group as a whole better, as measured on a simple 11-point affect scale, than was the case for members of leader-centered units. The average rating of the group as a whole was 3.62 scale points for one group-centered unit in this latter experiment, contrasted to 2.00 for its matched leader-centered unit, and 3.28 for another group-centered section, compared to 2.06 for the leader-centered unit matched with it. Both differences are statistically significant, with a P of less than .01.

To complete the case for the hypothesis, the influence of affect on perception was recently demonstrated in an experiment by Zeaman (11). Judgments by a naive S of extent of autokinetic movement were shown to be influenced positively by the judgments of a planted experimenter, a girl whom the S was known to be partial to, and apparently influenced negatively by judgments of a planted experimenter towards whom the S was known to entertain mild feelings of hostility.

As has been indicated, the individual in group-centered structure certainly likes his unit better. It can be suggested that it is for this reason that the individual judgments of other members of such a unit, and their average, for the length of the green rectangle, have more weight for the member of the group-centered unit, than the corresponding judgments and their average have for his colleague in the leader-centered unit. In short, the verdict of the group probably carries a greater affective loading for the member of the group-centered unit.

A simple test of this explanatory hypothesis might be the correlation obtained between the relative convergence on a perceptual norm (the amount of change in standard deviation expressed as a proportion of the initial standard deviation), and the average rating of the group as a whole by its members on an affect scale, for a number of groups. It might be expected that more relative convergence would be obtained in those groups whose members liked their group as a whole better.

Results from this experiment suggest that in the friendlier atmosphere of social interaction in the group-centered unit, the individual may differ more widely in initial opinions, attitudes, and judgments from other members of the group, until these areas of behavior become part of the zone where conformity is demanded in this particular group. If this should happen, however, and the group should take a stand or formulate a norm for a specific area of behavior, then this norm or verdict would weigh more heavily with the individual in influencing his behavior than would be the case for the leader-centered unit.

The present experiment suggests that, paradoxically enough, the group having the widest initial latitude of behavior possible is also one in which more conformity can be obtained from the individual in those areas where the group demands it. And it suggests that the fundamental process in creation of such a permissive, yet basically powerful, group is verbal interaction among its members.

SUMMARY

The present study was designed to test the hypothesis that greater modification of perception of an objective stimulus in the direction of a common norm following presentation of individual estimates and their average,

would be obtained in group-centered units than in leader-centered units. The principal procedural difference between these two kinds of group was that member-to-member verbal interaction was at a maximum in the former, and held to a minimum in the latter. The stimulus to be judged was length of a green rectangle.

The procedure was to ask the members of each group under test to estimate anonymously the rectangle length, then inform them of the individual estimates and the average for the group, and finally to ask them to re-estimate the length. The announced purpose of the experiment was to determine the effect of passage of time on accuracy of perception. The difference in standard deviations obtained between the first and second judgments was taken as a measure of convergence for the group.

A total of 504 *Ss* in 20 different groups was used for the study.

CONCLUSIONS

1. Individual perceptions of an objective stimulus by members of a group-centered unit will shift more in the direction of a common norm, when the individual judgments and the group average are made known, than will the perceptions of members of a leader-centered unit. Group-centered structure can thus be said to have more power to alter the perceptions of individuals in the direction of a common norm than has leader-centered structure.

2. The initial dispersion of perceptual estimates of an objective stimulus, before the individual estimates or their average is known, will be greater for group-centered structure than for leader-centered structure.

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COMPETITION: THE EFFECTS OF INFANTILE EXPERIENCE UPON ADULT BEHAVIOR[1]

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Various theories of personality (1, 4, 8) and especially psychoanalysis (3) emphasize the role of infantile experience in determining adult behavior. At present only a beginning has been made toward securing controlled experimental evidence supporting such theories. Hunt (7) has shown that food deprivation during infancy will increase the amount of hoarding by adult rats. The purpose of the present experiment is to investigate the effects of infantile experience on a different type of behavior, namely, competition. The social importance of competitive behavior is obvious. An attempt will be made to test the hypothesis that a limited period of hunger-motivated competition for food during the infancy of mice will cause them in adulthood to show increased competition for food even when not motivated by hunger.

A pilot study which preceded the experiment to be reported here, and other published results (2), indicated that the experience in deprivation per se was primarily responsible for the development of competitive fighting over food.

Problem

In the present experiment an attempt has been made to test the hypothesis that a limited period of competition for food during infancy will result in competitive behavior at a later stage in development, despite the absence of hunger.

Method

Population

A total of 50 inbred mice were used. These mice were the offspring of brother-sister matings well beyond 30 generations, and it may consequently be assumed that within a given strain the individuals were (except for sex differences) almost identical.

This group consisted of eight black mice of subline 10 from strain C57; twenty-two C57 blacks, subline 6; sixteen C Bagg albinos, and four C3H mice.

Each strain had an approximately equal sex representation. The animals were weaned at 21 days of age and lived in pairs, males with males, and females with females.

Procedure

The technique which elicited competition over food consisted of presenting a given pair of hungry mice with a single piece of hard laboratory food. The types of competitive response varied with the strain. Several strains were included in this study because pilot studies exploring the competitive behavior of these strains revealed characteristic differences in competition.

The albinos exhibit a primarily mild type of competitive response which consists of a huddling together and taking the food away from the competitor when needed. The C3H mouse (agouti) is an intense fighter but slow in becoming aroused. Both sublines of the C57 black mouse are vigorous fighters and also extremely possessive. A typical fight consists of a chasing of the opponent that has the food, culminating in vigorous wrestling, biting, and loud squealing. The writer (2) has isolated another form of fighting which is spontaneous in nature and apparently not a function of frustration.

The experimental design consisted of two separate experiments, each with its own littermate controls.

Experiment I. The experimental group was given seven one-a-day trials in competition beginning at 29 days of age. This group was retested when 72 days old. The first and crucial trial at this time occurred when the mice were not hungry. Presumably, these sexually mature adult mice would, according to the proposed hypothesis, fight over food though not hungry because of their experiences in competition during early life.

The control group, consisting of the littermates of the experimental group, received no experience in competition over food or food deprivation during infancy, but was tested when not hungry at 72 days of age. These mice should not fight over food because of the absence of previous experience in this kind of behavior.

Experiment II. This experiment differs from Experiment I in that the experimental group received only a single experience in competition over food at 33 days of age. This group was also retested when 72 days old.

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The control group for this experiment was tested at 72 days, having had no previous experience in competition over food. In order to prevent the possibility of conflict altogether, these mice literally lived and ate upon a cage floor padded with food. Presumably they should not compete over food when not hungry because of the lack of previous experience. In contrast to this, the control group in Experiment I was fed from food hoppers which permitted the eating of food but not its acquisition.

In both experiments competition, once started, was permitted to last for ten seconds after its onset; the mice were then separated by *E* to prevent dominance. The length of a trial was five minutes.

These two experiments can actually be considered as one, since the emphasis is on what the mice in both groups do in comparison with a control group at 72 days of age and not hungry.

TABLE 1

BEHAVIOR * OF MICE ON TESTS IN INFANCY AND ADULTHOOD, EXPERIMENT I

SUBJECT	AGE (IN DAYS) 29 D.†	30 N.D.†	31 D.	32 N.D.	33 D.	34 N.D.	35 D.	72 N.D.	73 N.F. 24†
Experimental Group									
1 C m	1	1	1	1	1	1	1	1	2
2 C f	1	1	1	1	1	1	1	1	3
3 C f	1	1	3	1	1	3	1	2	2
4 C m	1	1	3	1	1	1	1	1	2
5 C f	1	1	3	1	1	1	1	1	2
6 C3H f	0	1	3	3	3	3	3	2	2
7 C 57–10 f	3	2	3	3	3	3	3	2	2
8 C 57–10 f	3	3	3	3	3	3	3	3	3
9 C 57–10 m	3	2	2	2	2	2	2	3	3
Control Group (No experience in competition over food during infancy; never deprived of food.)									
11 C f								0	3
22 C m								0	3
33 C f								0	1
44 C3H f								0	3
55 C57–10 m								0	3

* KEY: O, disinterested; 1, mild competition; 2, possessive; 3, vigorous fighting over food.

† *D.* means that mice were deprived of 1 gram of food for 24 hours, out of a total of 3 grams, which is the average amount of food consumed by mice at this age. *N.D.* means not deprived in this manner. *N.F. 24* means no food at all for 24 hours.

RESULTS

Tables 1 and 2 indicate the experimental design, as well as the results for Experiments I and II, respectively. Upon combining the results as shown in the tables, we find that the critical test involves 14 experimental and 11 control pairs. The results indicate that the experimental *Ss* competed over food when 72 days old, although they were not hungry at the time. The *Ss* in the control group, on the other hand, did not show any evidence of competitive behavior. Attention should be directed to the fact that the numbers as found in both tables, namely 0, 1, 2, and 3, are not rating scores; they represent levels of competitive behavior which characterize the different mouse strains. The crucial point is whether competitive behavior, regardless of its specific behavioral form, does or does not occur. From a general point of view, the differences between mouse strains in terms of competitive behavior point to hereditary differences in the management and expression of aggressiveness.

A statistical evaluation of these results has been made by the X^2 test. Specifically, a test was made here in terms of testing the divergence of the obtained results from values computed on the null hypothesis. Thus, when the results in Tables 1 and 2 are combined, the proportion of competition obtained in the total population, namely, 14/25, is used as the proportion to be expected by chance in each cell. This procedure allows a test of the null hypothesis which considers the experimental and control groups as randomly selected from the total population. Computation shows the results for both experimental

and control groups to be significant well beyond the .01 level of confidence. The customary correction for continuity as it applies to small table entries has been made. One can, therefore, discard the null hypothesis and state that the results obtained are not due to chance, and probably indicate the effects of an infantile experience.

The advantages and disadvantages of sampling homogeneous populations are clearly revealed by these results. On the one hand, it is not actually surprising to find a literally 100 per cent conformity of experimental results with the originally postulated hypothesis, namely, that the effects of an infantile experience make themselves known at a later stage in development despite the absence of the originally existing need.

From a genetic point of view, one would, of course, expect a population sample with little or no hereditary variability to show homogeneous effects in behavior as well. The fact, therefore, that no exceptions occurred merely reflects the actual use of several inbred strains with restricted behavior responses. The experimental data may, however, for this reason represent an artifact, because some other mouse strains as yet unknown to us might have given completely different results. Several strains were used, specifically in order to test this possibility in a limited fashion. In general, the above considerations show the necessity for a meticulous account not only of environmental but of hereditary variables as well.

Tables 1 and 2 reveal, as an additional check of the experiment as a whole, that the control groups began to compete for food as soon as they were deprived of it, but not before that time. The importance of frustration for the elicitation of competitive behavior is also shown by the fact that the experimental group (Table 2) when first tested at 33 days of age, did not compete over food during the first "not-hungry" trial. In other words, all of the *S*s that were used showed that they were capable of competing in one way or another over food, but that an experience in competition during infancy caused the experimentals to compete later on, even when it was not necessary for them to do so.

TABLE 2

BEHAVIOR * OF MICE ON TESTS IN INFANCY AND ADULTHOOD, EXPERIMENT II

SUBJECT	AGE (IN DAYS)					
	33 *N.H.*†	34 24 h.†	72 *N.H.*	73 *N.H.*	74 24 h.	75 *N.H.*
			Experimental Group			
20 C 57–6 m	0	3	3			
21 C 57–6 m	0	3	3			
22 C 57–6 f	0	3	3			
23 C 57–6 f	0	3	2			
24 C 57–6 f	0	3	2			
			Control Group			
			(No experience in competition over food during infancy; never deprived of food.)			
10 C 57–6 f			0	0	0	2
11 C 57–6 f			0	0	3	3
12 C 57–6 m			0	0	3	3
13 C 57–6 m			0	0	2	3
14 C 57–6 f			0	0	3	2
15 C 57–6 m			0	0	3	3

* KEY: 0, disinterested; 1, mild competition; 2, possessive; 3, vigorous fighting over food.
† *N.H.* means not hungry; *24 h.* means 24 hours without food.

DISCUSSION

The fact that a single infantile experience produced results which persisted into adult life seems to support Guthrie's (5) contention that important learning can occur in one trial. The exact nature of the reinforcement for such learning is a problem. Can the present concepts of reinforcement theorists such as Hull (6) deal adequately with (1) the

reinforcement of such behavior on a single trial, and (2) the performance of such behavior by adult animals in the absence of the primary drive of hunger?

SUMMARY

The present study explored the importance of early experience in competition with respect to adult behavior. Hungry infant mice were trained to compete over food for a few days shortly after weaning. Following this experience they were allowed to grow into adulthood without having to compete. This experience in competition during infancy turned out to cause them to fight over food on a retest many weeks later when they were sexually mature; they were not hungry at this time. The littermates of the experimental *Ss* were raised without competitive experience during infancy. This control group did not compete for food when adult and not hungry. It is believed that these results support theories of personality which emphasize the importance of infantile experience.

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INTELLECTUAL MALFUNCTIONING AND PERSONALITY

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IT is one of the traditional tasks of the psychologist to evaluate the intellectual potentialities of individuals. The traditional tool for such evaluation is the intelligence test. At the same time, experience with the use of intelligence tests has shown that this instrument has serious limitations. If, for instance, of two individuals with the same IQ, one behaves considerably more "intelligently" outside the test situation than the other one, it becomes evident that intelligent behavior is affected by factors which may not be measured by traditional intelligence tests.[1] More specifically, these factors are thought to be personality traits, such as persistence, emotional stability, curiosity, etc. However, it would be erroneous to say that these personality factors affect the degree to which an individual behaves intelligently outside the test situation without ever influencing the scores on intelligent tests. That psychometric performance also may be influenced by personality factors is a tacit assumption made by clinical psychologists (62) and corroborated by various studies which submit the components of intelligence to factorial analysis (3, 9, 61).

Thus, a child with a comparatively high IQ, who functions on a low academic level, may be handicapped by personality factors unfavorable for high academic achievement. In this case, these unfavorable personality factors depress the individual's academic achievement more than they depress his psychometric performance. Stoddard (58) indicates that the nature of the psychometric situation prevents certain personality factors from having an effect on test scores, while the same factors may be of potent influence on intellectual achievement outside the test situation. The ability to concentrate one's effort and interest over a long period of time on a problem, for example, is a factor which may have little effect on psychometric performances, since each problem in such performances requires only a very short amount of time. On the other hand, the ability to make a persistent effort may be an important factor affecting intellectual achievement outside the test situation. In other cases, nonintellective factors may depress test scores as much as they depress performance outside the test situation. For example, this is the case with certain individuals who are described as being pseudofeebleminded. The test scores of such individuals fall within the feebleminded range. Their behavior outside the test situation is in agreement with the test scores, i.e., on a defective level. Yet, there may be indicators in these individuals' performance on tests and projective techniques as well as in their present and past behavior outside test situations which make the clinician suspect that nonintellective personality factors rather than low intellectual ability are responsible for the defective behavior. Such suspicion is, of course, of great practical importance to the clinician, since unfavorable personality traits may yield to therapeutic effort. Thus, in studying nonintellective factors inhibiting intellectual functioning, the psychologist should not limit himself to cases where he finds a discrepancy between psychometric score and performance outside the test situation.

It will be noted that no attempt has been made, thus far, to draw, by definition, a line of demarcation between intellective and nonintellective factors. Wechsler says "personality traits *enter into* the effectiveness of intelligent behavior and, hence, into any global concept of intelligence itself" (62, p. 82). If this is the case, the question may arise how these personality traits can be distinguished from "intellective" factors. Probably the only way to distinguish the two kinds of factors is by enumeration, not by definition. We

[1] The statement that the term "intelligence" as used in the field of intelligence testing has a different meaning from the same term as used in everyday language expresses the same idea in different words.

speak traditionally of the ability to do arithmetical computation or to grasp spatial relationships as intellective factors, while we do not include curiosity under the same heading. The recognition that intelligent behavior is influenced by every aspect of the personality may induce the psychologist to do away completely with any line of demarcation between intellective and nonintellective factors.

Part of the psychological literature on nonintellective factors affecting intelligence deals with the correlation of personality traits as measured by personality tests with intelligence as measured by intelligence tests (43). This is a perfectly legitimate and fruitful approach. But this approach should not exclude another type of approach, in which the problem of total personality and intelligence is not seen as a correlational investigation between isolated traits but as a causal investigation of the effect of various aspects of the case history of individuals on their intellectual functioning. The intelligence of individuals is studied as affected by parent-child relationship, and by the complicated dynamic interaction of impulses and defense mechanisms, by the vicissitudes of psychosexual development, and the like. It is highly important that we psychologists, the traditional evaluators of intelligence, should give this dynamic approach to the study of intelligence the emphasis it deserves.

It is the purpose of this article to survey some of the dynamic personality factors as this survey are case studies of individuals rather than large-scale statistical investigations. The individuals studied are handicapped in intellectual functioning by emotional factors. In some cases psychotherapy has greatly increased their intellectual efficiency. Here, as in so many instances, the study of abnormal personalities serves as a magnifying glass to help us take notice of factors which are present to a lesser degree in the normal personality. The same emotional factors which cause pathological intellectual blocking in some individuals may, by being present in different degrees, become responsible for quantitative and qualitative differences in intelligence within the normal range.

Another fruitful and "magnifying" source for getting acquainted with the influence of personality factors on intelligence are case studies of geniuses, i.e., of individuals with whom such factors play a strongly enhancing rather than a blocking role. However, such studies are not included in the present paper.

This article is a discussion of some emotional factors which have been identified in the course of intensive study of individuals as possible blocks for the unhampered functioning of cognition. In most cases a combination of these etiological factors, rather than one single factor, is responsible for intellectual blocking.

ETIOLOGICAL FACTORS [2]

Lack of Parental Reward

It is well known that the emotional relationship of parents with their children may influence the children's intellectual efficiency. Thus, parental rejection may be a factor which blocks the child in the progress of his intellectual development. There are various possible connections between parental rejection and learning disabilities. The author has pointed out one connection in a previous treatment (63), the main points of which are as follows:

Learning processes of any kind and at any age are connected with frustration of more or less serious degree. The child feels frustrated when he has to learn to drink from a cup instead of a nipple. It is frustrating, too, for him to adjust the functioning of his bowels to the demands and taboos of the society in which he lives. Also, the acquisition of knowledge and skills at school is frustrating to a certain degree. What is it, then, that makes children put up with all the thwarting "do's and dont's" which are impressed on them? It is obvious that children could never be induced to put on the tight corset of cultural demands unless they are given some reward which makes it worth while for them. Maybe the most powerful reward is love and approval, especially when given by the parents or parent substitutes. Thus, if the child develops intellectually, if he learns to master the three R's and to acquire the skill which enables him to perform the little tricks required by psychometric scales, he does it partly in exchange

[2] The sections in the following discussion are somewhat overlapping.

for approval, love, and security.[3] If he does not get this reward because he has no parents or parent substitutes, or because the responsible adults fail to give love and security, he may fail to learn what is expected from him by society and to develop intellectually in the desired direction. This is usually an unconscious psychological mechanism rather than conscious malingering. Such a child appears dull to the superficial observer. However, he may actually be like a good businessman, who does not deliver the merchandise unless the price has been paid; sometimes it is smart to be dull.

Thus, if we encounter parents who seem to reject their children because they are dull and do not get along at school, we find frequently that the cart has been put before the horse. The children may not be rejected because they are dull, but they may be dull because they are rejected.

Desire to Punish the Parents

In our culture the most potent satisfiers of children's needs, namely the parents, are, at the same time, the most potent frustrators. Such a culture can be expected to create a frequent desire in children to punish their parents. This desire may be present even though the relationship between the parents and children is a "normal" one. It may become strongly enhanced by such maladaptive factors as domination, neglect, etc. Because of societal taboos and the child's dependence on his seemingly powerful, omnipotent parents, the hostile impulses of children against their parents remain frequently unconscious. For the same reason, punishing actions against the parents have often to be undertaken in an indirect, camouflaged manner whereby the connection between the action and its punishing significance may remain entirely unconscious. The refusal to eat, or to defecate regularly, is often an unconscious way of punishing parents, characteristic for the preschool child, but often carried on far beyond the first years of life, and even into adulthood. The refusal to develop intellectually—for example, to progress at school—may have the same unconscious significance (4, 5, 6, 52). The following example illustrates the above.

Shirley, an 18-year-old girl, was referred to the author for psychotherapy because of her inability to meet the academic requirements of a junior college. Her Wechsler-Bellevue IQ was 120. She showed considerable blocking in her social relationships. She was overcome with feelings of despair and anxiety whenever she was in social contact with contemporaries, but she felt perfectly at ease with her parents and their friends. Her academic difficulties at school, however, were probably not a mere outcome of her inability to associate with people of her own age, since she exhibited the same blocking towards academic achievement when taking individual instructions from a tutor. Shirley's mother underwent psychotherapy simultaneously with Shirley. She was a woman who had great difficulties in accepting a feminine role. Her marriage and pregnancy forced her to give up her aim of getting graduate academic training and becoming a lawyer. She unconsciously resented Shirley for this reason. When Shirley reached school age and proved to have good intellectual endowment, her mother transferred her professional ambitions from herself to her daughter, hoping that Shirley would get a higher education and reach the goal which she herself had to give up. Shirley's father was a pediatrician and very fond of children. He wanted to have another child, a desire which met with strong resistance on the part of the mother. Shirley remained the only child. The father's arguments in favor of having a second child centered especially around the idea that Shirley's development might suffer by her being the only child. The mother tried to pacify her feelings of guilt about the matter by making herself believe that Shirley would associate with many other children and never be lonely on account of her onliness. Thus, the mother had two main ambitions for her daughter. She wanted her to get a higher education, and she wanted her to be a sociable person who is surrounded by and enjoys the company of a large number of contemporaries.

It is remarkable that Shirley blocked in exactly the activities which were most important to her mother, namely, in her association with contemporaries and in her intellectual growth. In the course of Shirley's therapy it became apparent that behind a façade of extreme submission she harboured a tremendous amount of resentment against her mother. The material brought out in the course of her treatment suggested that the two main symptoms of her neurosis were partially determined by her unconscious desire to hit her mother's two most sensitive spots.

Edith Buxbaum (12) cites another case where learning difficulties appear to be a device to punish the parents.

A girl with academic difficulties was constantly admonished by her mother to study. The girl stated that her mother thought she was unable to

[3] This statement does not negate heredity as a factor in the determination of intellectual differences. However, this article is concerned with the effect of environmental rather than hereditary factors.

do her school work without constant nagging. In the course of treatment the therapist advised the mother to stop any kind of interference with the girl's learning process. The mother followed this advice and stopped nagging, whereupon the girl remarked that she would now show her mother how wrong she was to think that her daughter would only progress in school under constant nagging. From then on the girl made good progress. The author comments that the girl was apparently able to make progress, or not to make progress, in order to spite her mother.

Desire for Self-Punishment

Like many other neurotic symptoms, intellectual blocking may be brought about by an unconscious desire for self-punishment and, thus, for atonement of guilt feelings (5). These guilt feelings may stem from some of the sources discussed in this paper, or from other sources. The fact that a very high prestige premium is put on intellectual achievement in certain strata of society facilitates the use of learning disabilities as a self-punishing device.

Individuals whose intellect serves in the function of self-punishment frequently torture themselves through many other devices besides failure; for example, by compulsive pedantry in their intellectual work, through exaggeratedly long studying hours, etc. (7, 34, 38).

Desire for Masochistic Gratification

Intellectual blocking may be unconsciously enjoyed as a masochistic pleasure by the afflicted individual. The desire to obtain such masochistic gratification may be among the etiological factors of pseudostupidity (30, 32, 39). As is well known, this desire may be a factor in the etiology of many other psychological disorders.

Desire to Maintain an Infantile Level of Gratification

Intellectual malfunctioning may be a manifestation of a conscious resistance against growing up. Every step which the child undertakes in his development toward maturity brings about the necessity to forego more infantile gratifications. Thus, developmental progress is often accompanied by considerable nostalgia. Growing up means losing the privilege of entertaining sweet, dependent, protected relationships which are based on self-centered receiving rather than on a give-and-take basis. For example, the satisfaction of passive, irresponsible sucking at the mother's breast has to be given up, first for the sake of more active, less convenient cup-and-spoon feeding, which lacks the intimate physical contact with the mother and burdens the child with heavy responsibility, and much later for the strenuous and inconvenient activity of making a living. Intellectual growth, too, means renouncement of the satisfaction of dependent needs. Learning to read may mean to a child not being read to (32); learning to think may mean not being thought for; learning to orient himself in his environment may mean not being guided and protected. The child who is excessively afraid of losing infantile gratifications may block in his intellectual development (39, 44, 51).

Mahler-Schoenberger (44) reports on an 18-year-old boy suffering from pseudoimbecility. His behavior was seriously retarded. He walked with a shuffling gate like an automaton and sat with his arms listlessly hanging at his side. His mother and his siblings felt toward him as towards a small child and treated him accordingly. They exchanged kisses and caresses with him, a form of behavior in which they could not have indulged if the patient were a normal 18-year-old boy. Mahler-Schoenberger considers the desire of obtaining such infantile gratification one of the etiological factors of the boy's neurosis.

In this connection it may be interesting to note that clinical workers are frequently concerned as to whether the mentally retarded child will encounter rejection by his parents and whether the parents will show favoritism towards the normal siblings. The opposite danger is discussed less frequently, namely, that the parents, especially the mother, may develop an unduly strong attachment toward the defective child, so that the normal sibling may be at a disadvantage. In many ways mentally retarded children may offer certain libidinal satisfactions to parents which the normal child cannot supply, namely, complete, helpless dependence without the threat of a second childbirth, i.e., of a painful separation from the child at the time of adolescence. Such helplessness may satisfy the parents' need to protect, or to dominate. In a more indirect manner it may also satisfy the parents' dependency needs, since it offers them the possibility of identifying with a completely dependent individual.

The parents may, thus, enjoy the care of a feebleminded child directly and vicariously. In this manner, *true* mental retardation may offer certain libidinal satisfactions to the child and to his parents. *Pseudo*retardation, on the other hand, may be caused by the unconscious desire of the child to gain such satisfaction, and unconsciously encouraged through the same desire in the parents.

A graduate student of the author who worked in an institution for the feebleminded remarked jokingly that if he would ever adopt a child, it would have to be a Mongolian imbecile. The remark was, of course, not serious, but humorous in a grim way. However, a genuine emotion may have given rise to his statement, namely, the sweet feeling of attachment which many adults experience towards creatures who are and will forever remain helpless. It is known that Mongolians with their characteristic clinging behavior often become the favorites of institutional staff members.

The previous discussion also throws some light on the problems of children who show intellectual regression, such as deterioration of speech, reading, or other mental skills, at the time of the birth of a younger sibling. At that time the advantages of the infant's way of life and the futility of trying to capture the parents' attention by "academic sophistication" seems especially apparent to the older child. If the child could express his feelings, he would say something like, "Why did I have to go through all the troubles of growing up, when this helpless and stupid baby receives so much affection?"

In this connection it may be enlightening to compare the attitude of adults toward children of very superior intelligence and toward less intelligent children. We often find that superior children are to a lesser degree recipients of adults' protective warmth. Many adults are inclined to handle superior children with objective detachment. Thus, the unconscious desire to avoid such curtailment in warmth and affection may result in intellectual blocking with children of superior ability. Gumpert (27) says about the American woman that "she would rather be loved than respected" (p. 163). The same may be true for some children of superior intellect.

Displacement of Attitudes toward Oral and Anal Impulses upon Intellectual Processes

The well-adjusted infant gets great satisfaction from the intake of food. The mouth seems to be the most important organ of his body. He may develop a variety of attitudes toward oral gratification, depending on the person who feeds him and the manner in which he is fed and weaned. His oral development may proceed unhampered or be subject to various disturbances. For example, he may fail to experience the normal satisfaction of feeding due to the absence of a secure emotional relationship with the mother or due to various other factors. In this case he may refuse the normal amount of food intake. In many instances, drinking and eating may become a tool by the help of which the child rewards or punishes his mother, or food intake may turn into an antidote against anxiety and insecurity. In the latter case the child may eat an excessive amount of food.

Similarly, defecation is an important source of pleasure for the child. During an early period in his life he likes to smell, touch, and smear his bowel movements. He may be as proud of his feces as an adult individual is proud of personal achievement. His mother usually indicates to him that it is a prerequisite for friendly acceptance in the family and society to acquire a somewhat negative attitude toward anal activities and anal products. She also trains him to limit his defecation to specific times and conditions. The well-adjusted child is able to give up his anal freedom without too much frustration in order to please his mother. However, the anal development can also undergo various disturbances. This may occur if the relationship between mother and child is an unfavorable one, so that the child is unable to give up anal pleasures for her sake. If, for example, she tries to enforce toilet training in a hostile, rigid, and nonsympathetic manner, the normal development may be seriously endangered. The child may become overcompliant or rebellious. In the former case he may adjust as far as overt behavior is concerned, but "boil inside" about the environment's impingement upon "his private life." In the latter case he may refuse to conform to anal etiquette and annoy his

mother by developing training difficulties. Constipation, for example, is at times a form of protest which enjoys great popularity among toddlers. No wonder, since it fulfills so many purposes at the same time. It is a symbolic expression of the child's unwillingness to give, a method of punishing the mother, a way of getting attention and feelings of power, and a secret device to increase anal stimulation.

Needless to say, the attitudes toward orality and anality which are acquired during early childhood may persist into adulthood (1, 2, 21). The gourmet, the drunkard, the worrier about digestion, the ulcer patient, the chronically constipated individual are examples of continuations of oral or anal conflicts. However, attitudes toward oral or anal matters can also be displaced upon certain other fields. Experience has taught us that displacement is often facilitated by similarity between the person, object, or situation *from* which displacement takes place, and the person, object, or situation *upon* which the individual displaces (49). For example, attitudes toward parents are more often displaced upon older persons in an authoritarian position than upon younger subordinates. Thus, attitudes towards orality and anality are most likely displaced upon activities which possess certain similarities with drinking, eating, and defecating. Intellectual activities show such similarities. Learning is similar to drinking and eating inasmuch as it is an intake and assimilation process. In reading and in listening to lectures, knowledge is taken in and assimilated. Intellectual activity is also a giving-out process, and as such it resembles defecation. Writing, answering the teachers' questions, etc., are examples of this aspect of intellectual work. According to psychoanalytic theory, various disturbances of oral and anal development may thus be displaced upon intellectual activities, and such displacement may result in intellectual disturbances (1, 2, 4, 25, 32, 36, 37, 55, 56, 59). In this manner, the refusal to take food may be transformed into the inability to grasp and assimilate subject matter. Likewise, the tendency to be constipated, when displaced upon intellectual functions, may result in unwillingness or inability to express ideas, for example, to answer questions, to recite in class, etc.[4]

Moreover, the similarity of intellectual activities to pregenital (oral and anal) sexual activities may make the individual feel guilty about intellectual work, and such guilt may block mental development.[5]

One of Bornstein's (7, p. 393) intellectually blocked patients made the following statement: "I lack milk for my brain, for my thinking" (translated by the present author). The remark may indicate a possible connection between orality and mental retardation in this case.

Chidester and Menninger (15) report on a boy, Henry, who received psychoanalytic treatment at the Southard School from the age of 12 to the age of 15 years. When treatment was begun, his IQ on the 1916 Binet was 65. In the course of therapy the IQ rose to 90. Oral and anal displacement was, according to the author's interpretation, one of the emotional factors responsible for his intellectual malfunctioning. He was especially weak in arithmetic and writing. He would irritate his teachers by his extreme slowness in giving answers. Before he underwent treatment he spoke so slowly that it sometimes took him 15 minutes to complete a sentence. However, he was a good and eager reader. His intellectual pattern was one of taking in without giving out. The same pattern could be observed also in nonacademic situations. His relationship to other children was one of taking without giving. He requested gifts from them, took their possessions, asked questions, but never volunteered any information. He had the habit of collecting old cans and other odd objects, which he never used. Instead, he liked to hoard them in his room and was deeply upset if anybody tried to remove them.

That Henry displaced oral attitudes upon the reading process was suggested by various indications. He was, for example, occasionally punished by being prevented from reading the newspaper. He stated that he then felt "as mad as a hungry bear" (p. 621). At such occasions he would look for discarded food in garbage cans. The interpretation that Henry's resistance against speaking or writing

[4] According to the psychoanalytic school, speech disturbances may be caused by oral as well as anal displacement. Speaking is performed with the mouth and, thus, invites oral displacement. On the other hand, it is an expulsion process and, therefore, lends itself to anal displacement.

If the displaced oral and anal disturbances have originated as a response to lack of emotional reward, as protest against growing up, or as a device to punish the parents, then we find an overlapping between the first, second, and fifth sections above and the present section of this paper. However, learning difficulties can arise as an effect of the previously discussed factors without the detour of oral and anal displacement.

[5] For such cases, the present section of this paper overlaps with the section immediately below.

is connected with anal retention gains probability from the fact that he exhibited so many adjustment patterns characteristic of the anal retention phase, such as hoarding. Similarly, he expressed his resistance against his analysis in a passively stubborn, rather than in an actively aggressive, manner. During periods of resistance he refused to groom himself. He also became constipated frequently and remained in the bathroom for hours even though there was a line of children waiting outside. All these are characteristically anal forms of behavior. One day he made an interesting statement with the purpose of explaining his reluctance to communicate with other people in and outside the school situation. He said he was hesitant to express ideas since that meant giving them away; the more you said, the less you retained for yourself. In his magical thinking he seemed to equate the expression of ideas with the expulsion of matter.

Displacement[6] *of Inhibitions from Specific, Threatening Aspects of Cognition to Intellectual Activity in General*

Inhibitions caused by guilt feelings. (*a*) *Sexual curiosity.* In certain strata of society the attitude of adults toward children's sexual knowledge is, in many ways, diametrically the opposite of their attitude toward other kinds of knowledge. This rather obvious fact becomes especially apparent if we examine the literature on sex education written for parents and teachers of young children. Many authors, for example, make statements to this effect:

A frank way of discussing the facts of life will, they say, stop the child's curiosity and interest in these matters and will put an end to his preoccupation and his questions. Now let us compare this alleged aim of sex education with the aim of any other branch of education. Is there any other field in which the teacher aims at squelching the children's intellectual thirst, at suffocating interest and curiosity, and at stopping further questions? On the contrary, educators measure the success of their teaching not so much according to the amount of knowledge or information they transmit, but according to the interest they create. Questions, stimulated curiosity, attempts to make further investigations are considered criteria of success in teaching. Thus, the aims of sex education appear to be the exact opposite of the aims of education in any other field (26).

It is no wonder that many children develop guilt feelings about their knowledge and curiosity concerning sexual matters. According to psychoanalytic theory, such guilt feelings do not always remain limited to knowledge about sex. Again, displacement upon similar stimuli may take place, i.e., the guilt feelings may spread to intellectual activity in general. Since the guilt is irrational, it usually remains unconscious, but may result in serious blocking of intellectual functioning (6, 52).

The author treated an 11-year-old boy who was making poor progress at school. His school achievement improved considerably after treatment for several months. The main topic of conversation during the counseling sessions was "the facts of life." The boy had very drastic misconceptions about reproduction. His foster parents had never given him any sex information. He had acquired most of his knowledge from an older boy, whom he met secretly, against the wish of his foster parents. He considered the conversations about sex which he carried on with this boy as "bad." The treatment sessions taught him that it was not necessarily bad to talk, to know, or to be curious about sex. This relief of his guilt feelings about a specific type of knowledge may be one of the factors which decreased his blocking against school work.

Sylvester and Kunst (60) report about an interesting fantasy of a boy with a reading disability.

He daydreamed that his father had invented a big machine and had cautioned the boy to stay away from it. It was a dangerous machine which made terrific noises. However, the boy disregarded his father's warning and looked at the machine. As an effect of this disobedience, he was forced to run with closed eyes for many years so that the machine could not catch him. He then pretended in his daydream that *he* was the inventor of the machine, and that he was not permitted to make any further inventions. The fantasy suggests that the boy feels guilty about curiosity, knowledge, and intellectual creativity, and that he believes it is safest to "keep his eyes closed." Since the boy is suffering from a reading disability, guilt may be one of the etiological factors of his difficulty.

Thus, the conventional parental reasoning—that if curiosity about sexual matters is suppressed, children will turn their thinking "to more wholesome and constructive matters"—may be a fallacy. In many cases the results may be the opposite from what the parents expect (see 56).

[6] In many instances where the term "displacement" is used in this paper, it would be more correct to use the term "displacement or stimulus generalization" since the exact nature of the mechanism cannot be determined. (See Miller [49] regarding the distinction between the two mechanisms.)

b) *Aggression and sadism.* The above discussion indicates that intellectual blocking may result from guilt feelings about a specific *kind* of knowledge and subsequent displacement upon intellectual work in general. Similarly, guilt feelings about a specific *aspect* of knowledge may develop and be displaced upon intellectual functioning in general. For example, the aggressive and sadistic aspect of thinking may, thus, become an instigator of learning difficulties (5, 6, 8, 22, 32, 33, 37, 38, 39, 40, 41, 54).

There are various connections between the acquisition of knowledge, and aggression and sadism. One possible connection becomes apparent during preschool age, when the toddler tears apart toys, such as dolls, or living creatures, such as beetles, in order to investigate what is inside. Also the desire to know what is inside the human body may become associated with the aggressive desire to tear apart and to destroy.

Sylvester and Kunst (60) report about an eight-year-old boy who was placed in a subnormal room at school even though he was of superior intelligence. His behavior during play therapy suggested strong aggression against the mother. He stated that the mother doll did not want to carry her baby since the baby might kick her. Finally the boy tied the baby doll to the mother doll's arm in order to prevent him from kicking. It also became apparent that the boy was filled with strong curiosity as to what was in his mother's body. During his play with dolls, he expressed the desire to tear the mother doll apart in order to see what was inside. Thus, intellectual curiosity and aggression were closely associated in the boy's personality structure. Furthermore, the situation was aggravated by the fact that the mother was seriously sick. The boy's magical thinking made his own aggressive desires responsible for her disease. In this manner, his guilt feelings were greatly increased. Finally, displacement of guilt from the aggressive aspect of "wanting to know" to intellectual functioning in general took place, and the boy developed a serious learning disability.

Aggression and intellectuality may become associated in various other ways. Intellective learning, for example, has a strong competitive aspect in certain strata of society. With some individuals competition may become the main motive for learning. Competition, however, implies aggression. Again, guilt feelings about competitive aggression may be displaced upon learning in general. Guilt feelings about competition are especially likely to arise in a culture where cooperative and competitive ideals are taught simultaneously. While tribes such as the Mundugumor stress competition and tribes such as the Arapesh preach mainly cooperative ideals (48), Western civilization finds itself in between the two contradictory ideals of competition and cooperation. Both attitudes are taught simultaneously. Thus, Western man is quasi forced to go out in the rain without getting wet. Indoctrinated with the ideal of competition, he finds it difficult to cooperate, and his training in cooperation makes him feel guilty about competitive activities.

In addition, some of the words used synonymously with "intelligent" and "stupid" suggest an association between intelligence and aggression in people's thinking, for example, the terms "sharp" and "dull." Similarly, Landauer (35) points out that the German word "albern," meaning stupid, is derived from the obsolete word "alvari," meaning good or friendly.

Since intellectual work is related to a higher degree to the masculine role in our culture then to feminine activity, women often use their intellect as a device to compete aggressively with men. If such competition becomes the major motivating factor of intellectual endeavor, the ensuing guilt may become a serious block to progress.

Especially strong guilt feelings about competitive aggression are likely to arise in the Oedipus situation and in sibling rivalry, since the victims of aggression in these situations are people who should be loved, and not hated, according to the mores of our society. Thus, it is not surprising that the etiology of pseudostupidity is often connected with Oedipal hatred (4, 7) and aggressive competition against siblings (37, 40, 41).

Bornstein (7) reports a case of a 12½-year-old girl with whom Oedipal competition resulted in guilt and intellectual retardation. Learning, for this girl, was partially motivated by the desire to gain the affection of her father and of men in general. She was convinced that her mother and teacher were afraid of her competition for the attention of men and, therefore, did not want her to progress intellectually. The girl interpreted any encouragement she received from her mother or teacher as an insincere, strategic device designed to give her a false belief of achievement in order to prevent her from further striving.

Thus, it seems that intellectual endeavor may become associated with, and work in the service of, various impulses. The fate of intellectuality will then depend upon the fate of these impulses. If the intellect functions "in the service" of aggression, and if the individual feels guilty about aggression and tends to repress it, the servant intellect may share the fate of his master—and intellectual blocking may result.

Inhibitions caused by failure. Serious failure in specific intellectual endeavors may result in inhibition of intellectual functioning in general (32).

A child may be unsuccessful in his first attempts to master academic subject matter, not due to lack of intellectual endowment, but due to more extraneous factors such as a bad teacher, or a visual or auditory defect. The discouraging experience may condition the child in such a manner that he remains intellectually blocked even after all obstacles have been removed, e.g., after he has been placed with a better teacher or after his physical defects have been corrected.

According to psychoanalytic theory, such blocking is especially frequently instigated by children's failures in their investigations about sexual matters. These investigations are especially liable to meet with failure, since most adults tend to increase the obstacles of such endeavors or, at least, not to offer much constructive help. Moreover, his own psychosexual immaturity often prevents the young thinker from finding the truth. Instead, he may lose himself in a maze of contradictory hunches and fantastic sexual theories (8, 18, 23). Such "first failure" may have "a crippling effect forever after" (23, p. 68).

Inhibitions caused by miscellaneous other threats. Guilt about knowing and the threat of failure are not the only factors which can cause negative attitudes toward specific types of cognition. Knowledge may become threatening in many other ways. In such cases also, the individual may avoid a specific piece of knowledge, and the avoidance response may become displaced upon other or all intellective activity.

A ten-year-old boy was referred for psychiatric treatment because of various neurotic disturbances. When his mother was pregnant with his sister, he had denied to himself the coming of the child, even though informational and observational clues should have led him to the correct conclusion. He patterned his behavior to the slogan, "What I don't know doesn't hurt me." Displacement of this response was a partial cause of his reading disability, according to the author's interpretation (60).

Also, Freud's little Hans may have become a victim of "protective stupidity" when he said about his baby sister that "her widdler [penis] is still quite small" (24, p. 155), since the knowledge that girls do not possess penises may be of considerable threat to boys (see 51). In a sarcastic footnote to little Hans's remark, Freud draws a parallel between Hans's error and an erroneous statement made by philosophers:

> We can go a step further in vindicating little Hans's honour. As a matter of fact, he behaved no worse than a philosopher of the school of Wundt. In the view of that school, consciousness is the invariable characteristic of what is mental, just as in the view of little Hans a widdler is the indispensable criterion of what is animate. If now the philosopher comes across mental processes, the existence of which has to be inferred, but about which there is not a trace of consciousness to be detected—for the subject, in fact, knows nothing of them, although it is impossible to avoid inferring their existence—then, instead of saying that they are *un*conscious mental processes, he calls them *semi*-conscious. The widdler is still very small (24, p. 155)!

Just as in Hans's case, the difficulty of the philosopher to accept fully the concept of the unconscious may be a defense against threatening insight. Hermann (28) illustrates a similar kind of blocking, using Semmelweis' great discovery of the causes of childbed fever as an example. Semmelweis was bewildered by his observation that many more women died in a maternity ward attended by physicians than in a ward attended by midwives. Finally he found the explanation: the physicians examined their patients after they had conducted autopsies and, thus, killed them by infecting them. This understanding led to the introduction of prophylactic measures against childbed fever. Semmelweis' colleagues showed considerable resistance against the new discovery. Semmelweis was ignored, ridiculed, and even fired from his position (16). Such resistance may be partly due to the fact that the knowledge of having killed some of their patients, even though inadvertently, was highly threatening to many gynecologists. One of Semmelweis' young collaborators was especially strongly

affected by this threat. He had treated a cousin of his some time before the discovery was made. The woman had died from childbed fever. Semmelweis' discovery made it clear to the young doctor that he had killed his own cousin. He committed suicide. One could speculate that the young doctor, even though he was aware of all the necessary premises, could hardly have discovered the cause of childbed fever himself, since the threat of the discovery would have resulted in blocking.

The statement that a stimulus might remain unrecognized because it is threatening may seem a paradox, since it is hard to conceive how an individual could respond to a characteristic of a stimulus, namely its threatening nature, without previous recognition of the stimulus. However, clinical observation of this phenomenon is in agreement with various experimental findings. Postman, Bruner, and McGinnies(53), for example, found that subjects recognize tachistoscopically exposed stimulus words less quickly when these words are connected with areas on which the subject puts little value than when they are connected with highly valued areas. Thus, a scientist may recognize words such as "logical" more quickly than words such as "wealthy." The question arises how this scientist would "know" that the exposed word belonged to an area of low value, and therefore build up defenses against the word before he was able to read it. A similar riddle arises in McGinnies' study (47), where it was found that subjects show significantly stronger GSR's during the *pre*-recognition phase of emotion-evoking words such as "whore" as compared to neutral words such as "apple" (see 11, 46). The protective stupidity of individuals who don't become aware of threatening material, as well as the paradoxical results of these experimental investigations, are covered by Bruner and Postman's statement that "the threshold for affective avoidance response is frequently lower than the threshold for veridical report" (10, p. 27). In other words, subjects respond to "emotional tones" of stimuli before they recognize other characteristics of the stimulus situation.

It is tempting to speculate on the implications of "protective stupidity" for clinical research. Some clinicians are especially attracted by research topics which are closely connected with their own unresolved personal problems. Thus, stuttering clinicians may become interested in speech defects, etc. Such personal involvement in the area of one's research may become a serious block. As is well known, insight into the etiological factors of his own neurotic disturbances may be threatening to the individual. Therefore, any material contributing to such insight may be repressed. The removal of such repression usually encounters considerable resistance and often requires deep-reaching and time-consuming psychotherapeutic procedures. Consequently, if a scientist engages in research to discover psychological phenomena in other individuals which are closely related to his own repressions, he may encounter considerable difficulties. Like the previously discussed clinical cases, he may become a victim of protective blocking. Much of the pseudo-efficiency and pseudoexactness of psychological research may be an expression of the unconscious desire to avoid discovery rather than to achieve it, i.e., to seek security in a blind alley and, at the same time, "cheat the ego" into mistaking the blind alley for a road leading to the goal. Some investigations of this kind remind one of the following cartoon [7]: An elderly woman is seen copying a painting in an art gallery. The huge picture shows a nude and luscious nymph courted by a faun; an incidental little bird is seen in the corner of the painting. The copyist, disregarding the essence of the original, copies the bird only, with perfect technique, in an exacting and painstaking manner.

On the other hand, personal involvement in the area of one's research may be a strong incentive and have a constructive effect on the progress of research. This is especially the case when the type of repression discussed in the previous paragraphs is absent or incomplete.

Personal involvement may affect the psychotherapist in a similar manner as the research worker. When a patient discusses conflictive topics which correspond to repressed problem areas in the therapist's personality, the therapist may unconsciously block the patient's progress, e.g., by diverting him to less threatening topics (42).

[7] The source of this cartoon could not be identified.

Desire to Possess "a Magic Cap of Invisibility" (44)

When analyzing the previously mentioned 18-year-old pseudoimbecile (44), Mahler-Schoenberger calls the boy's intellectual retardation "a magic cap of invisibility." Thereby she postulates a possible etiological factor of his disturbance. The term "magic cap of invisibility" refers to the "Tarnkappe" in the Niebelungen Saga, which made Siegfried invisible and enabled him, thus, to engage in various kinds of socially unacceptable behavior without being discovered. By the help of his cap of invisibility he could witness situations from which he would otherwise be barred, remaining unnoticed by others. Similarly, Mahler-Schoenberger's patient is made "invisible" by his pseudoimbecility, i.e., invisible as an adult male with normal impulses. This invisibility makes it possible for him to obtain surreptitious gratifications from which normal adults are usually excluded. For example, he is able to satisfy his curiosity about his brothers' sexual life. One brother and his sweetheart, a married woman, engage in heavy necking in the presence of the patient, who is considered too dull to understand. For the same reason, the patient is permitted to enter the bedroom where another brother and his bride are in bed with each other. According to Mahler-Schoenberger, this gain obtained from the retardation is one of the etiological factors responsible for the boy's pseudofeeblemindedness.

More generally speaking, children may gain various satisfactions of their curiosity by wearing the "magic cap of stupidity." They may attempt purposely to appear naive in order to encourage adults in carrying on conversations about tabooed knowledge in their presence. If, instead of conscious suppression of the behavioral manifestations of understanding, repression of actual understanding takes place, "the magic-cap mechanism" becomes an etiological factor of intellectual blocking.

Jones gives a vivid description of children's attempt to appear stupid in order to gain forbidden information:

> "When a mother chats with her intimate friend over various private topics, frequently the child will resort to the strangest devices in order to stay in the room and listen to the conversation. Then when someone remarks him, and by her looks insinuates a doubt as to the propriety of conversing in his presence, he will interrupt his innocent crooning over his toys and indulge in exaggeratedly foolish antics, to disarm as it were, the suspicions of the company—by convincing them of his thorough simple-mindedness and innocence" (31, p. 484).

By using "the magic-cap mechanism" of pseudostupidity, the individual can get away with the expression of other unacceptable impulses besides sexual curiosity, e.g., with the expression of aggression (20, 31, 32, 35). Landauer (35) reports about two children who showed much interest in the fact that a woman living on the floor above them was expecting a baby. On the day the woman gave birth to the baby, the children broke a window in their own apartment. The mother asked who had done it. She received the reply that the window had been broken by the stork, who had mistakenly tried to enter through the window of their apartment instead of flying into the apartment upstairs. At this reply the mother broke out in laughter. The children were not punished for the breakage.

Desire to Avoid Self-Evaluation

Some individuals fail intellectually because they do not make any effort to succeed. Such "lazy" individuals are often extremely ambitious. Their ambition may be so strong that they could not bear to become aware of their limitations. Since they do not make any effort to succeed, they can avoid getting a realistic and possibly disappointing conception of their capacity. Thus their laziness enables them to excuse their failure and to cling to the magical belief that they would be champions if they would care to participate in the contest (6, 17).

Desire to Be the Recipient of Love Rather Than of Envy and Aggression [8]

The desire to succeed and the desire to be liked by one's fellow men are frequently incompatible. Success in climbing the ladder of achievement is often accompanied by loss

[8] This section is, in many ways, closely related to the section, "Desire to Maintain an Infantile Level of Gratification." However, it is believed that the emphasis under these two headings is sufficiently different to justify separate discussion.

of love. The successful individual may endanger himself by becoming the target of envy and aggression. Such envy and aggression against a successful person may be attitudes which are actually existing, or they may be projections on the part of the successful individual. In other cases actually existing aggression may be exaggerated in the recipient's perception through the mechanism of projection. Three kinds of projection, as described by Cattell (14), may be at work to produce such distortion. One is projection in the psychoanalytic sense, a mechanism by which individuals attempt to remain unaware of their own ego-alien impulses by ascribing them to the outside world. Thus, especially people who themselves have strongly competitive, aggressive attitudes which they cannot accept, may perceive their fellow men as more aggressive than they actually are. Another type of projection is called by Cattell, "Projection of Press Required by Emotional State" (PRES projection). When this mechanism is at work, the individual may be fully aware of his own attitudes. However, he distorts his perception of the outside world in a way which helps him to justify his own feelings. Thus, Murray (50) found that girls whose anxiety had been aroused experimentally, perceived photographs of faces as being more hostile than they had perceived them before the arousal of anxiety. Similarly, the hostile competitor may justify his uncooperative attitude toward his fellow men by perceiving them as more hostile than they actually are. Or he may expect hostility as a retaliation for his own feelings and, thus, perceive expected rather than actual hostility. Finally, Cattell distinguishes a further type of projection, "Naive Inference from Personal Experience" (NIPE projection). People with limited knowledge about their fellow men may naively assume that all individuals have attitudes similar to their own. This type of projection may also be applicable to the hostile competitor's view of the world around him. He may naively shape his perception of other individuals according to his own image.

Thus, the successful competitor may experience discomfort due to perceived hostility, regardless of whether his perception is realistic or distorted. He may attempt to avoid discomfort by avoiding achievement (29, 54).

A very bright student of the author excelled in class by her stimulating contributions to the discussion, and by the quality of her written work. However, after a few weeks of class work her contributions dropped noticeably in quality and quantity. In conversation with the author she stated that several remarks made by her colleagues were reported to her, indicating that her intellectual superiority made her unpopular with her classmates. Thus, she decided to control her behavior at school in such a way that she would appear less outstanding. We would hesitate to designate this girl as a case of intellectual blocking, because her plan not to excel was conscious, intentional, and reversible through deliberate decision. However, the study of individuals' attitudes toward success in intellectual activities or in any other field indicates that the tendency to "hold back" in order to avoid hostility can lie anywhere on a continuum from complete conscious intention to entirely unconscious blocking. For example, "holding back" may start as a consciously planned maneuver and later develop into an automatic, uncontrollable habit.

Brilliant, well-educated, and sophisticated young women occasionally engage in diplomatic malingerings when associating with men in social situations such as dates. They try to hide their knowledge and sophistication in order not to be threatening to their companion and, thus, to enjoy the pleasures of unambivalent male affection rather than to be frustrated by the ambivalent emotional relationship of competitors and the pseudo-triumph of Pyrrhic victory (32). In other cases, the expectation of loss of gratification may automatically inhibit their intellectual development, without conscious planning or even against conscious planning. Every step which a man undertakes toward vocational success makes him more desirable as a love object, but every step undertaken by a woman in the same direction may make her less desirable as a partner for love and marriage. This conflict between two goals may become an etiological factor for the automatic inhibition of activity directed toward either goal.

The role played by the innocent, unsophis-

ticated fool in myth, fairy tale, and fiction may serve as an illustration of the statement that intellectual blocking may be caused by fear of hostility. The fool in folk literature and other fiction is often an appealing figure, loved by everybody, envied by nobody (35). Since he is at the bottom of the ladder already, nobody can push him down.

CONCLUSION

The above is a discussion of some connecting bonds between intelligence and total personality. The discussion claims neither completeness nor originality. The emotional factors affecting thought processes covered in this paper have been described by others. However, the treatment of these topics is scattered through the psychiatric and psychological literature and interwoven with various other material. The author considered it a worth-while undertaking to extract pertinent material from various sources in order to present a more systematic discussion of intellect and total personality. Moreover, much of the subject matter surveyed in this paper is, in its original source, expressed in the often highly esoteric language of psychoanalysis, and interwoven with other psychoanalytic material which may be less acceptable to psychologically trained clinicians. In the author's opinion the above presentation discusses important aspects of intelligence, with emphasis on dynamic connections rather than quantitative comparisons. This approach has been relatively neglected by psychological investigators. Yet the objective research training of the academic psychologist is badly needed for the study of these aspects. Most emotional factors affecting intelligence have been ascertained "intuitively" rather than by objective methods. It will require all the ingenuity of psychologists to plan research designs which corroborate or disprove some of the "hunches" discussed in this paper.

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CHANGES IN ATTITUDE THROUGH COMMUNICATION

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During the last two decades rapid strides have been made in the analysis and measurement of social attitudes. One leading contributor to this field has been our chairman, whose early research on the differentiation of general and specific attitudes (3) remains an important landmark. His study and those of a number of other leading social psychologists were carried out in the laboratory of Gordon Allport, whose name is inseparably linked with work in this area (1). The extensive work on attitudes has served first of all to provide us with greatly improved instruments of measurement, without which scientific study of attitude formation and change would be impossible. Here we are greatly indebted to the pioneer work of Thurstone and his associates (13), and to the recent contributions of Louis Guttman (6). There has also been accumulated considerable information about the relationship between attitudes and such factors as education, socioeconomic status, religious background, etc. Additional data are supplied by the experimental studies which have been carried out to assess the effect of various types of experience on attitude. In this category belong studies of the impact of college, foreign travel, contact with minority groups, and similar experiences. From the available studies of attitude, we now have, therefore, at least a preliminary knowledge of how attitudes are learned through direct contacts with people and events. Much less is known about the acquisition of attitudes in the absence of direct contact, where the attitude toward an individual, event, or idea is derived second-hand from others through communication. This is the area with which the research I am reporting this afternoon is primarily concerned.

Our interest in this topic was first aroused by the practical problems we faced during the war in attempting to assess the impact of various types of mass communication on the attitudes and opinions of soldiers. We found in the literature a number of studies showing that attitudes could be changed by communication but practically nothing bearing on the factors responsible for change. From our own researches in a military setting we attempted to derive generalizations concerning the effects of mass communication upon attitudes (8), but we were greatly limited by the fact that our findings had to be derived primarily from the analysis of the effects of communications which had already been prepared by the military authorities. Many of the conclusions, therefore, had to be of an ex post facto character. Whenever possible, we sought opportunities for setting up experiments with communications especially prepared to test various theoretical issues, and we found that such researches turned out in the long run to have more relevance to practical problems than studies undertaken to answer questions of immediate urgency. But the pressure of other work and the constant necessity to secure answers to problems of immediate concern to the military made us long for the opportunity to concentrate to a greater extent on theoretical formulation of problems in communication and attitude change and to subject to rigorous experimental testing some of the implications derived from such an analysis.

Our desire to proceed in these directions led to a grant by the Rockefeller Foundation, which provides us with this type of opportunity. Following the completion of the report on our wartime studies, *Experiments on Mass Communication,* we have begun work on a program of basic research on the experimental modification of attitudes and beliefs through communication. We feel that this area of investigation provides an unusually favorable research site for studying the higher mental processes, since in the study of attitudes one has a unique opportunity to see reflected the influence of both

[1] Presidential Address, Eastern Psychological Association, delivered on March 30, 1951.

rational and emotional factors, and the interaction between the individual's own past experience and the norms of the social groups to which he belongs.

Considerable thought was devoted to determining the type of organization most suitable for the prosecution of this kind of research. In view of the variety of patterns which are currently being explored, and the lively interest in research organization suggested by the letters I receive asking for information about our program, it seems worth while to give a brief description of our type of organization.

The first characteristic of our research program is that it is set up as a cooperative research and study-group rather than as a centralized, hierarchically organized, "project." Coordination is achieved through conferences and seminars, and each individual member of the group has freedom within the broad limits of the over-all program objectives to design and execute his own phase of the research in line with promising hypotheses and his own interests. Thus the work I shall be describing is the work of a number of individuals, and this paper is in a real sense a group report. The members of the group whose studies will be discussed include: Irving Janis, Harold Kelley, Arthur Gladstone, Edmund Volkart, Arthur Lumsdaine, Muzafer Sherif, Leonard Doob, and Fred Sheffield, at a senior level, and Herbert Kelman, Russell Clark, Jepson Wulff, Wallace Mandell, Elaine Bell, Seymour Feshbach, Walter Weiss, Marvin Herz, and Bert King at a junior, research-fellow level.

In the second place, the research cuts across conventional scientific disciplines. The topic of social communication is a natural meeting ground for interests of many behavioral sciences. Our group, consequently, contains not only experimental, social, and "personality" psychologists, but also representatives from sociology and anthropology. It is hoped that, as the program develops, political scientists interested in changes in political attitudes, and economists interested in the origin and modification of values will become members of the group.

A third characteristic of the organization involves the policy that the senior staff have university appointments for both teaching and research, rather than for research alone. The difficulties other projects have had when the research staff was divorced from the teaching function of the academic community influenced our thinking on this point. But it was also our feeling that the organization of materials for teaching provides valuable stimulation for research. Such a combination appears particularly important in an area in which no quick results are likely to be obtained but in which a long-term program of research appears necessary. Perhaps where routine testing of previously developed hypotheses is involved, it is possible that mass production research methods may be preferable. We have instead adopted a pattern very similar to the Faculty Research program of the Carnegie Foundation, which provides, for the majority of the staff, half-time research opportunities during the year and full time during the summer.

Related to this last characteristic is the definition of the objectives of the program as being both in the fields of research and training for research. It has been our observation that a high proportion of research projects are less than optimally effective in training individuals for research because assistants are employed primarily to do routine work. We attempt to have every junior staff member and graduate student obtain a well-rounded experience with the various phases of the research, rather than have him concentrate on the phase that is most efficient strictly from the point of view of research productivity. For this reason the assistantships are in the nature of fellowships that provide time for collaborative or individual research; and the students participate in the planning, design, administration, analysis, and writing-up of the research. The amount of responsibility of these researchers increases from the first year, which is often on an apprentice basis, until the last year, at which time they may select a relevant topic for a dissertation and thus be almost completely responsible for the execution of a study.

A month-long conference, somewhat similar to those sponsored by the Markel Fund and administered through the Social Science Research Council, is held during the summer. Its function is primarily to review the

work of the preceding year, and to plan new studies. It is proposed to invite each year for the summer conference several researchers from other institutions who are working on similar or related problems, as an antidote to isolationism. During the coming summer Theodore Newcomb and Samuel Stouffer will be visiting members of the group. Throughout the year, weekly or bi-weekly seminars are held to review the status of projects, to analyze theoretical problems, or to discuss work in adjacent areas having a bearing on our work. Every individual responsible for a study discusses it at each stage of its development with a three- or four-man committee. This procedure not only provides expert criticism but also serves to complement and supplement the research skills of individual investigators.

So much for organization. We may next consider briefly the objectives and methods of the research. The deeper we get into research in the area of attitude change, the more we become aware of the fact that there is no way to mark off this area from the whole of psychology. We must inevitably examine problems in perception, motivation, learning, emotion, concept formation and thinking, personality, and social relations. It is clear that the field of attitude change does not constitute a separable province of knowledge. Nevertheless, there are several characteristics of the present program which distinguish it somewhat from other programs of communication research. In the first place, the emphasis is on theoretical issues and basic research, in contrast to the large number of attitude-change projects which have an "action" orientation. Practical problems are worked upon only when there is some clear indication that they will be useful in contributing to the formulation of or the solution to a theoretical issue. The results are, consequently, equally relevant in their application to individuals interested in changing the attitudes of others and to those interested in avoiding being influenced by others.

Almost all of the studies employ controlled experiments, in which the various factors affecting attitude change are isolated through specially constructed communications. We hope in this way to supplement the numerous researches going on elsewhere that use surveys and correlational analysis. Our experience has certainly been in support of Lewin's oft-quoted remark that you never really understand a phenomenon until you can produce it through a change in conditions. Mainly because of the greater possibility of rigid experimental control, the communications that we employ are primarily restricted to the one-way variety: a fixed communication addressed to the audience is employed, and interaction between the recipients of the communication is restricted. While our main efforts are directed toward experimental manipulation of variables, it would be presumptuous to expect that all propositions can be submitted to experimental test immediately. It is inevitable that extensive case study analysis and survey methods are necessary to define an issue more sharply or to ascertain whether the conditions which are theoretically required for a particular outcome are realistically present. But the objective in utilizing these nonexperimental methods is primarily that of clarifying hypotheses for later experimental test, or for preliminary exploration of the nature of the variables involved in a complex situation.

As an example, we are currently interested in the effects of preparatory communications in modifying subsequent attitudes toward stressful or frustrating situations. Before beginning experimental studies, one of the members of our group found it advantageous to devote considerable attention to studying, under field conditions, how people reacted to the stressful experiences of surgical operations. Intensive case studies, based on interviews before and after operations, were made in order to discern the ways in which reactions to postoperative frustrations and deprivations were influenced by expectations which had been built up beforehand as a result of various types of preparation given by physicians and friends. After preliminary study of this type, a number of hypotheses emerged which could be tested experimentally. One such experiment, currently being conducted by Janis and Herz, uses a simple frustration situation in which precise observation can be made of the effects of alternative forms of verbal preparation that are given to different experimental groups.

From the illustrative researches to be de-

scribed, it will be seen that there is considerable eclecticism in the type of theory involved, doubtless derived from the fact that members of our group have had their training under such diverse systematists as Lewin, Hull, Allport, and Lasswell, and research participants have included theorists like Doob, Sherif, and Newcomb. I am sure, nevertheless, that it will come as no surprise to hear that we have a strong predilection for stimulus-response learning-theory formulations and that an attempt is made to see how far the general principles of behavior theory can be extended into this field.

Looked at from the standpoint of general behavior theory, attitudes are viewed as internalized anticipatory approach or avoidance tendencies toward objects, persons, or symbols (cf. also Doob, 4). They are habits, and the general principles of learning should be of aid in understanding their acquisition and modification. For example, we expect that factors such as temporal gradients of reinforcement are applicable, and hence we should be able to make predictions concerning the strength of an attitude as a function of the time interval between elicitation and reinforcement. Such an application of learning theory is of utility in explaining the greater effectiveness, for most purposes, of individual as compared to mass communication (Hovland, 7). As another example, the principle of stimulus generalization suggests that similar attitudes will be held toward individuals who are perceived as being similar and toward individuals to whom the same responses are customarily made or the same labels have been applied. As with other habits, motivation must be a most significant factor affecting the acquisition and strength of attitude. In the case of social attitudes, the motives to affiliate with other individuals and groups are particularly significant determinants. Studies of group affiliations consequently are of critical importance. We are interested in the way prior attitudinal habits affect the acquisition and modification of new habits. This is the attitude-change counterpart of the classical problem of transfer of training. In some cases it is sufficient to deal with circumscribed patterns of pre-existing attitudes, but in others the total pattern of response habits for the individual is relevant. In the latter case, the analysis can be described as the study of the influence of "personality" on attitude change. Formulation of the attitude problem in terms of behavior theory, then, requires interest to be focused on the intervening symbolic processes that mediate the acquisition, differentiation, and extinction of attitudinal responses. Studies of these mediating processes are an important phase of our program.

Having outlined the organization and objectives of the research, I should like in the remainder of my talk to describe briefly a half-dozen or so research problems growing out of the present approach to the problem of attitude change. This description will constitute a "progress report" rather than a final summary of work completed. Some of the topics involve research already in press, while others include research that is currently under way or in the late stages of planning.

I. Since attitude change has been formulated as a learning process, the role of motivation becomes central. Basic preferences and aversions are acquired from life experiences in which the individual's needs are satisfied or frustrated. In parallel fashion modifications of attitude through communication result from the arousal of motives mediated through the use of symbols and their subsequent satisfaction or dissatisfaction. Motives may be expected to operate in a variety of ways, e.g., by increasing the individual's attention to the communication, by provoking rehearsal of the content, by providing for selective reinforcement of the desired verbal response, by instigating the transformation of verbal responses into overt action, and by creating persistent motives which operate outside the communication situation. In our studies to date we have been most concerned with the effects of the communication in arousing powerful acquired motives and with the influence such arousal has on the amount and kind of effect produced by the communication.

A great deal of fundamental research is required in the area of motivation before adequate measurement of the effects of various types of communication on motives is possible. Exploratory studies of the effects of motive-arousal through communication

upon responses to the Thematic Apperception Test are being conducted by one of the members of our group, Russell Clark, along lines developed by McClelland and his collaborators (11). Interesting differences result from arousing socially inhibited motives (e.g., sex) as compared with those that are socially approved.

Several studies have been completed dealing with the effects of anxiety-arousing motives in attitude-change communications. In line with ordinary principles of learning it is assumed that unpleasant affective states are evoked as a consequence of anticipations of painful consequences and that therefore the communicatee is motivated to try out various responses, including symbolic responses. It is further assumed that these responses are determined in large part by the previous experiences of individuals in successfully eliminating or reducing similar affective states. Whatever response terminates or reduces the intensity of the aroused emotional tension is reinforced and therefore tends to become a habitual response to various cues present in the communication situation. One of the most frequent types of response that reduces emotional tension is symbolic rehearsal of means of averting the threat. When a mass communication arouses anxiety anticipations in an audience, the beliefs which are most likely to be learned are those whose content embodies some means of averting the threat. If, for example, one arouses anxieties about the possibility of developing fatal, incurable cancer because of failure to detect a growth early enough, but then produces reassurance by suggesting a regular check-up at a tumor detection clinic, the communicatees will learn to practice the appropriate resolution whenever they think about cancer. If, however, the anxiety is not sufficiently reduced by the reassuring recommendations, it may motivate unintended responses such as avoidance, distraction, and failure to pay attention. This is likely to occur if too strong arousal is elicited by a communication which continually plays up the magnitude of the threat.

Some of these factors were studied by Feshbach and Janis. They utilized three intensities of anxiety appeal in a standard communication concerning the proper type of dental hygiene habits. One group was given a lurid description of the consequences of failure to use the proper toothbrushing methods, illustrated by vivid photographs of tooth decay and gum disease taken from books on dental pathology. A second group was given the same rules of dental hygiene but was told about the consequences of tooth neglect in purely factual language and with much milder illustrations. The third group was also told the proper techniques but without any special anxiety-arousing material.

The results indicate that strong anxiety arousal has detrimental effects with respect to building sustained attitudes. From a questionnaire given to all of the groups one week after the communication, it was found that the group exposed to the strong anxiety appeal was least likely to report a change in their actual dental hygiene practices and that, moreover, it apparently was much more vulnerable to subsequent counterpropaganda. After a brief statement that minimized the importance of proper toothbrushing technique, this group was much more likely to agree with the statement than were the others. In fact, the greatest resistance to the counterargument was displayed by the group which had been exposed to the least amount of anxiety-arousing material. These findings are consistent with the following hypothesis: the more strongly anxiety is aroused about the dangerous consequences of failure to adhere to recommendations, the greater the motivation to accept competing ideas which reduce anxiety by minimizing the importance of the threat. Thus, when anxiety is so strongly aroused and is not sufficiently reduced by reassuring recommendations as to what can be done, people may learn to ignore the threat.

The type of theory used in this connection has relevance to many current information programs. One thinks immediately of the extensive program designed to arouse the public concerning the consequences of the atomic bomb. During the early days of the program high anxiety appeals were employed without suggesting the types of behavior that would serve to reduce the anxiety. Some detrimental effects apparently did occur—loss of interest in the subject and a tendency to ignore the anxiety-producing threat. It is

likely that the civil defense material currently presented in pamphlets, newspapers, and radio programs is having a more positive effect: it concentrates mainly on telling people what to do and does not especially emphasize the dangers to be anticipated.

II. A second area of research is concerned with the ways in which attitudes are affected by references in the communication to groups of which the individual is a member. The close relationship between the influence of the social group and the area of motivation just mentioned is readily seen, because of the enormous power of both formal and informal groups to provide various satisfactions for their members or to withhold them. Our general interest is in the factors which affect the extent to which membership in groups serves to maintain attitudes in the face of competing pressures to change. A number of studies in this area are being conducted under the general direction of Harold Kelley and Edmund Volkart.

One such study is using boys' clubs to test the proposition that the more a member values his membership in a group, the more resistant to change will be those of his attitudes which are anchored in the norms of the group. Here club members from groups of high or low cohesiveness are exposed to a communication which dissents from the beliefs held by the groups. The problem is to determine the extent to which members of highly cohesive groups resist the influence of the communication as compared with those in the low cohesive group.

One of the problems in this area in which we are most interested is that of the internalization of social attitudes. Under what conditions does a group member really come to accept the common attitudes as his own, and when does he merely conform overtly to the group norms but privately express attitudes at variance with those held by the group? The relevance of the individual's relationship to the group in forming attitudes is being tested by comparing the attitudes expressed in groups of high and low cohesiveness under two sets of conditions: one where the attitude is registered in private and anonymously, and another where the attitude is indicated in public before the group. The theoretical prediction is that the difference between public and private expression will be more marked in the low cohesive groups than in the high ones.

Other studies are concerned with the extent to which an individual, in any given situation, is aware of the *different* groups of which he is a member. During the experiment, a given group and its norms can be called to the attention of individuals in varying degrees. For example, an attempt to change attitudes of Boy Scouts toward various Scout norms might be made through a communication during a regular Scout meeting; or through communication to the Scouts while they are in school, without mentioning the fact that the issue is of any concern to the Scouts; or through a school communication containing the reminder that the topic is one on which Scouts have already indicated a consensus.

These studies in turn point to some interesting problems of the *saliency* (10) of group norms and their relationship to attitude change. Since resistance to change is probably lower when the group's position on an issue is not salient, attempts will be made by communicators to try to change attitudes with no reference to the fact that the position being advocated is in conflict with that of the individual's own group. What are the subsequent effects of re-exposure to the norms of the group after the change? What is the outcome of the conflict? An experiment is under way in which an issue is presented on which the group has a position but where membership in that group is not very salient to the members. In one variation, no mention of the group is being made; in a second, the group is brought clearly into the discussion. Measures will be taken immediately after the conclusion of the communication to determine the relative resistance to change under these two conditions. In addition, later measurements will be made after many of the individuals will have become aware of the conflict between the newly acquired attitudes and the position of their group. The nature of the resolution of the conflict can then be determined by analyzing those who become aware of their group's position.

Another approach to the problem is represented by a study in which attitudes were modified by changing directly the relation-

ships between members of the group, thus modifying the motives of the individuals in the group. A study in this area has been completed by Sherif (12), jointly sponsored by our research project and the Department of Scientific Research of the American Jewish Committee. A site was secured near New Haven for a summer camp completely controlled by the research project. Twenty-four boys were selected from towns in Connecticut to represent a moderately homogeneous sample of lower middle-class children of about twelve years of age. During the first phase of the study all of the boys were treated as one large group in which a variety of different kinds of activities permitted contact with every other member and in which the spontaneous groupings of the subjects were studied by sociometric and observational techniques. After this period, the camp group was divided into two subgroups that were chosen in such a way as to split as much as possible the spontaneous groupings developed in the preceding period. The two groups lived in separate bunkhouses, sat at separate tables, engaged in separate games and other activities. The activities selected required cooperative participation by all the members of the subgroups. The effect of these subgroup activities was to shift radically the attitudes of the boys toward other boys in the camp. Sharp declines were obtained in the number of expressions of friendship for boys in other groups, and there was an increase in friendship expression for members of their own group. In sociological terms a strong ingroup had developed, and all of the boys' attitudes were oriented toward the new grouping. In the third phase of the study the subgroups were placed in opposition to each other; competitive games were played in which the members of the winning side were rewarded as a group. In other cases situations were devised so that the behavior of one of the groups caused severe frustration to the members of the opposing group. Marked evidence of increased ingroup feeling and outgroup hostility was obtained, and members of the outgroup tended to be characterized in most unflattering terms. In the final phase of the experiment an attempt was made to bring all the boys together to reduce the antagonism and leave them in a mutually friendly frame of mind. The amount of difficulty involved was evidence of the deep attitudes developed by the earlier experience. It is hoped that follow-up studies can be made of the procedures most effective in reintegrating groups which have been split by division into opposed groups. From studies of this kind we hope to obtain further insight into the factors influencing group identification in the acquisition of social attitudes.

III. A third area which is a basic problem for the entire research group is the analysis of the central internal process occurring in attitude change. Adequate analysis must specify the role in attitude change of the different aspects of the process which go by such names as perception, conception, reasoning, symbolic process, implicit verbalization, and so forth.

This is indeed a most difficult area to study because the process cannot be viewed directly, and unfortunately it is rarely possible to make clear-cut differential predictions from opposed theories. As a consequence much of our work in this field must be exploratory. In many of our studies we attempt to interview subjects concerning their initial perception of the communication, and attempt to have the subjects verbalize as completely as possible their thoughts as they respond to the communication.

One hypothesis which has developed from this analysis is that each individual may have a somewhat unique or idiosyncratic symbol sequence which will occur consistently when he responds to particular persons and symbols. Unless new responses can be attached to these sequences, changes in attitude may not be brought about. One implication is that *stereotypes* are mediated through symbol sequences of this sort, perhaps through the utilization of labels in common. In a study being conducted by Arthur Gladstone, subjects were asked to describe what they think would be the personality characteristics of a group of individuals whose photographs are shown one by one. Intensive interviews were then conducted to determine the extent to which the subjects categorized the pictures by race, sex, nationality, or other basis as a preliminary to assigning personal characteristics. (Do they say to themselves, "This is

a Negro and *hence* lazy?") He found there was a tendency of this type; to accentuate the effect, therefore, he had some of his subjects first label the picture and then do the rating, and other matched subjects first rated the characteristics without overt labelling. The preliminary results support his hypothesis that evocation of one of the conventional labels tends to cause the ratings of personal characteristics to have a more stereotyped character. His technique permits one to determine the extent to which each subject has an idiosyncratic stereotype, as well as to analyze the more conventional socially shared stereotypes. The ultimate interest in these studies is in the effect of various ways of breaking up stereotypes and of dissociating labels from individual judgments.

Throughout this area it repeatedly happens that research on attitude change is retarded by the absence of adequate experimentation on an even more basic level. As a result, it is frequently necessary to detour from a main problem long enough to obtain information on an underlying issue. An important problem at the attitude-change level, for example, concerns the mechanisms by which some attitude changes are widely generalized to new situations, while others are modified in the communication situation but do not appear to function in even a closely related life situation. Analysis clearly shows the operation of two important factors: (1) motivation common to the original and transfer situation and (2) concept formation adequate for seeing implications in the new situation. But a clear-cut learning analysis of the conditions making for the formation of concepts adequate to generalize to new situations is lacking; consequently, laboratory experimentation is under way to determine the factors influencing the kinds of concepts formed under various stimulus sequences and combinations. Similarly, certain higher-level phenomena depend on a better understanding of the relationship between language and perception. In this category are the experiments being set up by Hovland and Lounsbury, an anthropologist-linguist. Subjects are taught the names of various symbols which have some basic characteristic in common. The technique is similar to that used in concept-formation experiments. On the test trials further examples of the symbols of the class whose name has been learned will be tachistoscopically presented and the accuracy of reproduction from memory studied. These data will permit analysis of the way in which the learning of a label affects the perception and memory of the material.

A study relating verbalization and attitudes is being carried out by Janis and King. They are investigating the alleged phenomenon that an individual is swayed by arguments which he is compelled to defend. It is said that salesmen often become convinced of the merits of their own product by presenting the sales arguments to customers and that debaters come to believe in the side they defend. But in a naturalistic life situation there are so many possible factors and mechanisms which could produce this result that even if it occurs, the explanation remains unclear. These researchers are attempting to isolate the critical elements by starting with a simple situation where the only difference between the experimental and control individuals is their assigned role in presenting a topic. A situation is used with the ostensible purpose of appraising certain aspects of the performer's public speaking talents: his ability to give a convincing talk to a small group of fellow-students. Sample topics—such as the effects of television on the movie industry—are assigned, and the experimental subject presents a talk based on a short outline furnished by the experimenter which contains the general arguments supporting a position. All subjects study the same material, and the experimental and control groups differ only with respect to whether they subsequently present or listen to the topic. Later on it is planned to add other elements which probably occur in real life situations, such as approval or disapproval reactions from the group to whom the talk is given. If mere presentation of the material proves to be a critical factor, more analytic procedures will be devised to analyze the basis for its effectiveness. It is possible that just saying something often enough causes one to believe it. So we jokingly refer to this area of investigation as a study of the "Saying is Believing" phenomenon.

From another study results bearing on this

topic are also being obtained. Herbert Kelman has been studying the effects of pressuring subjects into responding in various ways and seeing how this affects their attitudes toward the issues underlying their response. He finds that subjects who are required to conform to a particular position on an issue change their attitudes in the direction of the forced choice to a greater degree than those not made to conform. The extent to which this happens depends, however, on the type of pressure that is used. The effectiveness of different types of pressure seems to be related to the kinds of implicit verbalizations that they produce. Under one condition, subjects tend to make supporting and self-motivating verbalizations while conforming; under another condition, they tend to make interfering verbalizations. The former group shows significantly more change when measured at a later date.

Included in this general area are studies on the reasoning process and conclusion-drawing. It has been asserted by researchers in psychotherapy that conclusions reached by the client are more effective and permanent than those suggested by the therapist. Is there a corresponding phenomenon in the mass communication field? Are conclusions drawn by the audience more persuasive and long-lasting in their effects on attitude than those drawn by the communicator? The experiment set up by Hovland and Mandell to test this may be briefly described. In one version of a communication the arguments and supporting material were presented with the appropriate conclusion drawn by the communicator. In the other version all features were identical, except that the conclusion was not actually drawn. After considerable preliminary work, inductive and deductive methods were ruled out because any naturalistic communication constantly implies the conclusion. Instead, a "symptom and diagnosis" type of conclusion-drawing was developed. In this procedure antecedent conditions (or "symptoms") are described, and drawing the conclusion consists of associating the right diagnosis of the situation with the set of antecedents. The topic chosen was the desirability of monetary deflation, which was devised with somewhat oversimplified economics. A set of principles relating the effects of increasing or decreasing the value of money was presented, with discussion of conditions that make one or the other alternative desirable. Two contrasting examples illustrating the application of the principles under differing sets of conditions were supplied; the Australian experience was used as an example of conditions which made deflation effective, and the German situation following World War I as an example of conditions in which adverse consequences would follow. The conditions currently applying in America were then discussed, so that conclusion-drawing consisted of matching the described conditions with the pattern which would lead to either desirable or undesirable effects and thence to the conclusion that the U.S. should or should not change its monetary policy. For the majority of the subjects the drawing of the conclusion by the communicator was found to be more effective. It was also expected that the differential effect of the two types of communication might be affected by the perception of the communicator's motives; if a communicator has a motive to favor a particular conclusion, his failure to state his conclusion might be regarded with suspicion. Accordingly one version introduced the speaker as head of an exporting firm, who would of course be affected profit-wise by changes in the exchange rate, and the other version introduced the speaker as a leading economics professor. The results indicate an over-all advantage for conclusion-drawing by the communicator. Failure to state one's conclusions was found to be particularly serious if the motives of the communicator were suspected. Among some groups at least, omission of an explicit statement of one's conclusion may serve as a cue that the communicator has something to conceal.

IV. A fourth area concerns the influence of past experience on attitude change, a problem partly analogous to that of transfer of training for simpler habits. One study is devoted to the problem of how prior communication affects the response to a new communication about world events, since the way in which an event is interpreted when it occurs is frequently determined by the way in which the individual was prepared for it. Two years ago an experiment

was set up in a large Connecticut high school to study various factors influencing the formation of new opinions. The particular topic concerned Russia's atomic bomb development. One group had been given a communication designed to make the audience expect that Russia would not be able to produce a large stockpile of A-bombs for a long time to come. The control group received a communication on an entirely unrelated topic. But the scope of the study was drastically changed by an unforeseen occurrence. Three months after the initial communications had been presented, President Truman made the announcement that Russia now had possession of the atomic bomb. Here we had the ideal situation for observing the effect of a preparatory communication on the reception of a major news event. By returning to the same school and measuring the students' opinions immediately after the announcement, we were able to determine whether the impact of the event was reduced or augmented by the initial communication. The outcome has a direct bearing on an important issue in the field of public opinion. On the one hand, preparatory communications which build up optimistic opinions, as in this experiment, might have the effect of making people more resistant to accepting the implications of "bad" news. But, on the other hand, when people have been influenced by a preparatory communication containing optimistic arguments that are definitely shown by subsequent events to be false or inadequate, the "bad" news might be all the more impressive. The results indicate that the former effect is the one that was actually predominant. For instance, on questions concerning the length of time before Russia has *large numbers* of atomic bombs, the group which had received the communication three months earlier gave significantly longer estimates than the control group. Parallel results were obtained for expectations concerning the imminence of war and for other beliefs which were also influenced by the communication. In general, all of the findings support the hypothesis that people are less likely to be influenced by a major news event, in forming new expectations about the future, if they have recently been exposed to a preparatory communication which had built up opinion responses that are incompatible with those ordinarily elicited by the event. This study was carried out by Doctors Irving L. Janis, Arthur A. Lumsdaine, and Arthur Gladstone.

Other aspects of the same problem are being analyzed, particularly in relation to the amount of anxiety generated. This dovetails with a larger interest in the preparation for anxiety-producing events. Considerable other work has been done on the reactions of patients undergoing surgical operations who have had different types of prior expectations generated by the statements of doctors and friends as to the seriousness of the operation and the amount of pain involved. It is anticipated that further extensions of this work will help to develop a general theory that has some bearing on ways and means of preparing individuals for the possible effects of atomic bomb attacks against this country and for similar problems of interest to civil defense planning.

V. A fifth area of research is the investigation of the duration of the changes in attitude brought about by communication. In our wartime researches we obtained the puzzling result that under certain conditions attitudes showed greater change when a period of time had elapsed after the communication than when tested immediately afterwards. Several hypotheses which we advanced there are being tested. One is concerned with the effects of forgetting the source of a communication. In a study by Hovland and Weiss news stories on controversial issues are attributed to writers and magazines regarded by the subjects as highly reliable or notoriously unreliable sources. In half the cases, one side of the issue is supported by the source and the opposing arguments are refuted; the reverse procedure is followed for the other half. From the results it will be possible to analyze the extent to which (1) the source tends to be forgotten, (2) the forgetting of the source affects the points retained, and (3) high and low credibility sources influence the learning of the content. Delayed effects will be studied to determine whether attitudes once changed tend to return to their initial position, and whether such tendencies are greater when the communication is in conflict with the sub-

ject's initial opinion. In a related study Walter Weiss is seeking to ascertain whether the labelling of material as "true" or "false" by the communicator tends to be forgotten with the passage of time and to be assimilated in line with one's initial predispositions and motives. In another study, by Elaine Bell, the principal interest is in the relative rate of loss for the final conclusions derived from the presentation as compared to that for detailed supporting evidence and the effects of a differential rate on attitudes. One of the findings from her study is the tendency for the subjects to forget the cautions and reservations of the communicator and greatly to overextend his generalizations. Many interesting problems are unfolding in our use of the Bartlett (2) memory methods to study the retention of opinions and attitudes.

VI. In nearly every study there is an interest not only in the over-all effect of different factors, but also in the variation from individual to individual in response to an identical communication. Personality predispositions are accordingly a central problem. In the study of Feshbach and Janis on the effect of anxiety-arousing appeals, personality questionnaires were employed to study the way that initially anxious individuals were affected by the communications as compared with those less apt to be anxious. And in the study by Hovland and Mandell of the relative effectiveness of communications in which the communicator does or does not draw the conclusion, the questionnaires administered some time before the experimental procedure contained a number of questions designed to determine the extent to which the individual is suggestible and is influenced by the opinions of others, or not suggestible and highly individualistic in his judgments.

How individual differences in initial position on an issue affect the amount of attitude change is being studied by Jepson Wulff. He assumes that changes can occur only when the new response is already in the individual's repertoire. Thus, if one is attempting to change a person's belief about the number of hours of sleep required, an individual who believes that eight hours is desirable but that one can often get along on seven, and maybe under some circumstances on six, can more readily be changed from an eight-hour estimate to a six-hour estimate than the individual who believes eight hours is desirable and that six is never feasible. Thus his predictions are in line with the formulations made from perceptual theory about "assimilation" and "contrast."

One characteristic of the individual which is almost always found to affect the impact of communication upon attitude is intellectual ability. In our studies in the military setting we found that brighter individuals were more likely to have their opinions changed by a communication than were the less intelligent when the opinion was based largely on factual information. When, however, the *acceptance* of the material was the principal determinant of attitude change, the less intelligent were more apt to be influenced, since they tended to accept practically anything communicated to them. In contrast, brighter individuals were a great deal more skeptical. Our current interest in the intelligence variable relates primarily to symbolic mediation of attitude change. In our study of conclusion-drawing by the communicator as compared with conclusion-drawing by the audience, we are analyzing the results on the basis of the following hypothesis: While conclusion-drawing may be more effective when carried out by the more intelligent members of the audience, there is a strong possibility that the less intelligent will fail to reach the appropriate conclusion when they themselves must draw it, and that for them, consequently, conclusion-drawing by the communicator is more effective.

We are currently seeking a situation in which we can expose the same individuals to a series of different communications on a variety of topics. This will make it possible to isolate the individuals whose attitudes are changed by practically every communication regardless of subject matter, those who consistently fail to respond positively to any of the communications, those who tend to respond negatively to practically every communication, and those who are changed by some of the communications but not by others. If these different subgroups are isolated, study will be directed at the different psychological make-up of those within each of the groups, in order to discover per-

sonality characteristics which predispose them to be influenced or uninfluenced by mass communication.

I hope that even this brief summary of our research serves to indicate the wide range of interesting problems which exist in this field. The study of the impact of communication on attitude seems an excellent way to study the interaction of thought and emotion, the individual and the group, and the relationship between verbal and other types of behavior. But this is not to say that there are not serious difficulties confronting the researcher in this area, and a discussion of our topic would not be complete without mentioning some of these.

The first difficulty which strikes the experimentalist who turns his attention to this field is the type of theory involved. In the older traditional field of experimental psychology, theories are usually formulated with sufficient detail and precision to make head-on collisions of theories frequent, permitting crucial experiments. In social psychology, on the other hand, it appears that theories are usually of a highly general character, primarily concerned with comprehensive description. A few sample theoretical propositions from one of the best systematic texts in the field will illustrate this point: Krech and Crutchfield (10) give as three of their major propositions the following:

> The dynamics of molar behavior result from properties of the immediate psychological field (p. 33).
>
> The trend of behavior often involves progressively "higher" levels of stable organization of the psychological field (p. 66).
>
> The cognitive reorganization process typically consists of a hierarchically related series of organizations (p. 117).

Proof or refutation of such general propositions seems out of the question.

At times it almost appears that experimental research is somewhat superfluous in this field, since the generalizations in any good social psychology textbook cover the phenomena so plausibly. But I am now convinced more than ever that experimental research can make an important contribution to this field, and that more precise formulation of alternative theoretical explanations will lead to really debatable expectations whose resolution will stimulate new experimentation, and ultimately lead to new theoretical development. Pending the formulation of more comprehensive theories, theory construction has somewhat of a hand-to-mouth character: the experimenter must fashion a working miniature system with a limited number of theorems just a step or two ahead of his experimentation.

A closely related difficulty is the matching of theoretical variables and experimental operations. Theories to cover traditional experimental psychology have for a long time been designed along operational lines so that the variables are initially defined operationally. This is far from the case in attitude research. The hard thinking that goes into the process of converting abstract theoretical terms into rigorous operations is greatly needed. This is a particularly difficult problem because of the extent to which central concepts are of the "intervening variable" or "construct" variety, like "motivation," "expectation," and even "attitude" itself. I am greatly impressed, however, by the imagination and ingenuity which have been shown by the leading craftsmen in social psychology in devising techniques and situations which appear to match the conceptual variables, and I am convinced that experimentalists can profit greatly from social psychologists in this respect.

The phenomena in this area, to be sure, tend to be a great deal more complex than those in traditional experimental psychology. This situation introduces difficult problems of adequate control. A fine balance is required to introduce the experimental variables in a realistic manner and at the same time keep irrelevant factors under control. In many cases artificial control and simplification is absolutely essential if the basic variables are to be tapped. At the same time it is necessary to be constantly on one's guard against the type of simplification which destroys the initial phenomenon of study. Germane to the complexity is the wide range of research in related fields which has a bearing on problems in attitude modification. Many an experimental psychologist can keep reasonably abreast of his field through reading two or three journals, but the student of attitude change must cover large segments of psychology, sociology, and anthropology for relevant

material. This may account for the impressive erudition of our social psychologist colleagues.

Another difficulty which is much more acute in this area than in conventional experimental psychology is in generalizing about the stimuli used, the responses changed, and the population to whom the results apply. In general experimental psychology one deals with moderately simple stimuli which can readily be reproduced by other experimenters. In social psychology, on the other hand, the stimuli are often quite complex, and one must define the whole population of stimuli which are to be characterized in a particular way and use a sampling procedure from this universe to be able adequately to generalize about the stimulus used (8, pp. 7–8). For example, if one wishes to generalize about the effects of communications presented by a low-prestige source, one cannot use a single source, but a sample of satisfactory size drawn from a well-defined universe of low-prestige sources.

The same problem exists on the response side. Generalizations about the effects of various factors on attitudes (in general) can therefore only be made when an adequate sample of different attitudes carefully defined are studied. In addition, items designed to test a single attitude must be an appropriate sample from a carefully defined universe of items (6). Finally, we must specify the universe to which our generalizations shall apply and draw our sample accordingly. Most of us want our results to be applicable to people in general but do our experiments on a few classes in elementary psychology in our own college. In contrast to the low variability that exists in sensory processes, there is a high degree of variability in social-psychological relationships. For instance, from one culture to the next, and from one social class to the next within a culture, there are pronounced variations, and even within a small subculture wide differences in ability and personality result in great variability in response to complex social stimuli. Defining the universe to which our results apply is accordingly a serious problem for social-psychological research.

A final problem stems from the fact the influences which are of central importance in attitude change are of a long-term, cumulative character. We face a dilemma: the isolation and control of variables can best be achieved with single, controlled events, but the impact of a single experience may be insufficient to change the attitudes in which we are interested. We face the same difficulties as the researcher in child development, where control of formative factors over a prolonged period of time poses severe methodological problems. Lazarsfeld has said:

> If we visualize a learning experiment where people walk in and out as they please, where some of the most valuable effects are achieved with people who come in by mere accident, where the motivation to learn is often very low and where the possible rewards for learning are obvious neither to the experimenter nor to the subject—if we really think through such a situation quite seriously, then we can understand the frame of reference in which the cultural agencies of a modern society must perform their task (9, pp. 6–7).

While I would not of course agree with the implication Lazarsfeld draws for the limited value of learning theory, he does point out dramatically the difficulties of research in this field.

The consequences of the factors which I have mentioned are a great increase in the risks undertaken by the researcher in this field, and a slower rate of scientific progress. Anyone with adequate training can be assured of securing usable results if he sets out to determine the constants in an equation relating number of extinction trials to number of hours of food deprivation. In the area of communication and attitude research, however, the risks are considerable, and many a well-planned experiment will come out without significant results. When the experimentalist is critical of many of the articles he sees published in the social-psychological field in which inconclusive results are reported, he ought at least to be aware of the long period of exploratory work which has been required to bring about the standardized methods and critical experimentation he takes so much for granted in his own field. One hazard is that the investigator is testing out the implications of theoretical propositions which he himself has developed as a guide to his research. Under these circumstances he may be lauded if positive results are obtained; but if the results are negative,

the reader may say that there was little reason to predict the outcome expected by the researcher and that therefore disproof of his private hypothesis constitutes no real contribution to knowledge.

Even when he is attempting to test what is recognized as an important theoretical point, moreover, the investigator is hampered by the lack of adequately standardized tools of research. Characteristic of research in social psychology appears to be the fact that each investigator devises his own instruments and methods, so that comparability becomes a serious problem. The researcher in the social field lacks the sones, mels, and photons, as well as the Macbeth illuminometers, T-mazes, and Skinner boxes which are such a natural part of work in other branches of the field. Or an otherwise excellent experiment fails because the communication, which was designed to represent different degrees of some variable in whose effects we are interested, does not produce a strong enough impact to permit analysis of differential effects on subgroups. At present the actual writing of communications is more of an art than a science; hence, even if the theoretical formulation is good and the device for measuring effects is satisfactory, the experiment may not succeed.

Still there are signs that seem to me to justify considerable optimism concerning future work in this field. There is a growing tendency to make theoretical formulations more rigorous and testable. A good illustration of this trend is the recent article by Festinger (5) setting forth some of the conditions which affect the needs of individuals to communicate. A second reason for optimism is the increasing tendency to conduct experiments with the same instruments and procedures so that direct comparisons are possible, rather than to have each study employ its own unique methods and measurements. One of the most promising aspects of group research, as contrasted with the research of isolated individuals, is to increase opportunity for a number of investigators to employ closely comparable techniques in successive studies. And lastly, there is an increasing tendency, among those who conduct research on the problems of attitude change through communication, to utilize the theories and techniques of a number of diverse disciplines. Cooperative attack on problems in this area by psychologists, anthropologists, sociologists, and related fields will do much to ensure the ultimate integration of the theoretical concepts of these fields, and thus provide the bridge between the analysis of principles of communication and the conditions in society under which these principles apply.

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A TENSION INDEX OF ADJUSTMENT BASED ON PICTURE STORIES ELICITED BY THE MICHIGAN PICTURE TEST

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This is the third of a series of papers presenting the findings of the standardization process of the Michigan Picture Test, a projective personality technique suitable for use with children eight to fourteen years of age. The first paper, presented at the 1950 meeting of the American Orthopsychiatric Association at Atlantic City, dealt with the validation of several of the variables used in the analysis, including age and adjustment differences and diagnostic and therapeutic implications. The second paper gave an appraisal of the stimulus value of the cards selected for the test (1, 2).

This test was developed out of a comprehensive study which was designed to provide a valid measure of maladjustment as well as to provide normative data on emotional reactions of children as given in picture stories. The study is a cooperative research project conducted by the research division of the Michigan State Department of Mental Health, with the active assistance of the state child guidance clinics and public schools in a number of Michigan cities and towns.

The method of selection of the pictures was reported in the first paper. Here we recapitulate that the test stimuli were selected to sample crucial areas of personal-social adjustment in which children frequently have problems. After extensive preliminary study, 21 pictures, including a blank card, were retained. These 21 cards were divided into two series of 12 cards each, with four pictures ("core" cards) common to both series. The extra, or twenty-first card, is used for boys only, on one of the series. In the standardization process, only one series was given to each child, with the two series being alternated between every other child. The test instructions were brief and simple, somewhat similar to the Thematic Apperception Test (TAT) method. All tests were administered individually with electrical recording of the stories, which were later transcribed for scoring and interpretation.

In this paper we summarize the rationale for our procedure and then present some preliminary findings on the effectiveness of a "tension index" in discriminating between gross adjustment groups. It was the aim of this phase of the study to develop an objective and global index of maladjustment based upon the verbalized conflicts in the stories. Published articles on the TAT and other related tests show considerable differences in the definition, scope, and methods of analyzing the content of stories, with much of the analysis of the dynamics of conflict material usually left to clinical skill and intuition. Those who have worked with the TAT method in clinical practice know how difficult and time-consuming it is to fit the content of story material into extensive lists offered in the literature, which include all kinds of needs, drives, press reactions, various ideational categories, etc. And, when this has been done, there is a limited amount of normative data available to aid in interpreting the findings.

Clinicians, in their approach to analyzing the adjustment problems of an individual, generally assume that conflicts arise from basic unresolved needs and result in psychic tensions. One might ask whether the verbal expression of an unresolved need is evidence of emotional tension. It might be expected that the more maladjusted a person is, the more verbal expression of tension he would manifest. However, unless normative data are available for the specific stimulus cards used, one cannot determine whether or not

maladjustment is indicated. In order to investigate these questions empirically, we have attempted to develop a simple index of psychic tension that assesses the degree of adjustment on the basis of verbal expressions of unresolved psychological needs. We have called this the "tension index."

METHOD

The general procedure was: (1) to determine what psychological needs are elicited by a common group of pictures, (2) to find which of these needs discriminate between well-adjusted and poorly adjusted children, (3) to assign weights to each need in relative proportion as it appeared in the well-adjusted and poorly adjusted groups, and (4) to secure the tension index for each child by taking the sum of the weighted scores for his stories. Finally, the index was tested on new groups of well- and poorly adjusted children, and additional validation was secured by a comparison of a well-adjusted school population with a clinic population. A measure of interjudge reliability was also obtained.

The list of needs analyzed was selected on the basis of clinical judgment and included the following seven categories of psychological needs: (1) *love*, or the need for affection, belonging, or affiliation; (2) *extrapunitiveness*, including oral, physical, or other psychological aggression; (3) *intropunitiveness*, including fear and guilt reactions; (4) *succorance*, including dependence, security, or praise; (5) *superiority*, with reference to power, achievement, ambition, etc.; (6) *submission*, as indicated by deference, passivity, self-abasement, etc.; and (7) *personal adequacy*, either physical or psychological. When any one of these seven needs was verbally expressed in a story for a given picture, a tally was made for that need for that child. A separate tally was made for the intensity of the need, if any of the following general conditions were present: (1) the need was repeated more than once in the story by the same expression; (2) the need was repeated more than once in the story but by a different expression; (3) the need was accompanied by a long pause in the story, either before, during, or after the expression. The sum of the tallies for each child's story for each picture for each need thus gave us seven raw scores on the simple expression of needs, and seven raw scores on the intensity of needs.

The next step was to compare the raw scores of the well-adjusted and poorly adjusted children. These adjustment groups were selected on the basis of teachers' evaluations of children's objective behavior manifested in the classroom. These evaluations, which were obtained in the form of rating scales of pupil adjustment, were discussed in a former paper (1). The scale will be published with the test. Those rated as the upper third of a particular age group selected from the project's total school population were called the well-adjusted group, and those rated in the lower third were called the poorly adjusted group. Thus, each of the selected experimental samples consisted of 15 well-adjusted and 15 poorly adjusted children. All children in the samples were in the fifth grade. The two groups were equated for sex and geographic location. A third group included 15 children of similar age who were under treatment in several of the Michigan state child guidance clinics.

The weights assigned to the various psychological needs were determined on the basis of the relative proportion of occurrence of the need in the protocols of the two adjustment groups. The sum of the weighted scores for needs on the four core-cards constituted the tension index.[1] A score for the intensity of needs was obtained in similar manner.

TABLE 1

COMPARISON OF MEAN WEIGHTED TENSION SCORES BETWEEN GROUPS OF 15 WELL-ADJUSTED AND 15 POORLY ADJUSTED FIFTH-GRADE SCHOOL CHILDREN ON FOUR PICTURES

GROUP	EXPRESSION OF NEEDS				INTENSITY OF NEEDS			
	MEAN WT. SCORE	DIFF.	t	P	MEAN WT. SCORE	DIFF.	t	P
Well Adjusted	12.2	6.1	3.39	0.01	12.1	5.4	2.71	0.01
Poorly Adjusted	18.3				17.5			

RESULTS

The significance of the difference between the mean weighted scores for the two adjustment groups was obtained. The basic data are shown in Table 1. Here we find that the difference between the means on the expression of needs is significant at the 1 per cent level. The ranges in the weighted scores show a negligible amount of overlapping between the two groups. The difference in the means on the intensity of needs was also found to be significant at the 1 per cent level. However, scoring the intensity of the needs

[1] The four core-cards, which are administered to all children, have been found to be highly successful in screening maladjusted children. These cards and their uses are discussed more fully in prior paper (2).

contributed no further refinement in the selection of poorly adjusted children, and therefore this phase of scoring was eliminated from the final analysis of the tension index.

The tension index, which was thus found to be significant, was next subjected to cross validation. An entirely new population was obtained in which a representative sample of 15 well- and 15 poorly adjusted fifth-grade children was included. Again the four core-cards were utilized. For this validation group the mean weighted score on the tension index on the core pictures was 12.1 for the well-adjusted children, and the mean for the poorly adjusted children was 16.6. The difference of 4.5 is significant at the 5 per cent level. These results confirm the ability of the index to discriminate poorly adjusted from well-adjusted school children.

As a further check on our findings, we compared the poorly adjusted group of the new population with the well-adjusted criterion group of the experimental population. Again, the four core-pictures were used. In this comparison, we found that the mean score on the tension index was 12.2 for the well-adjusted group and 16.6 for the poorly adjusted group. The difference is significant at the 5 per cent level. It is also worth noting that the mean on the four pictures for the experimental group is almost identical with the mean of the cross validation sample, 12.2 and 12.1, respectively. These results further substantiate the discriminative ability of our index. We regard the effectiveness of the four cards in discriminating between adjustment groups as an important finding since it indicates that we may save much time in analyzing protocols for purposes of screening and routine diagnosis. Of course, the clinician may wish to supplement the core cards with the other cards for analysis of types of conflicts and the like.

Having found that our well-adjusted school group was a good criterion group on which to base our index, we next compared this group with 15 clinic cases of similar age. The results are shown in Table 2, both for the expression of needs which we now call the tension index, and the intensity of needs. The difference between the means on the tension index is significant at the 1 per cent level. This shows that our index discriminates effectively between well-adjusted school children and clinic children.

Again referring to Table 2, we note that even with clinic cases the score on intensity of needs is no more effective than the tension index, which is in line with the results given in Table 1.

TABLE 2

Comparison of Mean Weighted Tension Scores between 15 Well-Adjusted Fifth-Grade School Children and 15 Clinic Cases of Similar Age on the Four Core Pictures

Group	Expression of Needs				Intensity of Needs			
	Mean Wt. Score	Diff.	t	P	Mean Wt. Score	Diff.	t	P
Well Adjusted	12.2	11.7	3.02	0.01	15.1	4.5	2.64	0.01
Poorly Adjusted	23.9				19.6			

It is recognized that an important question is the replicability of our scoring system. To test the interjudge reliability of the tension index we used two clinicians differing considerably both in intensity and type of training. Using our list of definitions and examples, these clinicians independently scored the protocols of a group of ten children selected from our files at random. The cases scored were different from those included in the validation study. The correlation of weighted scores on the tension index obtained by these two scorers was .77.

Summary

We have developed a simple tension index based on psychological needs verbalized in story content. The success of this index in discriminating maladjustment in children, as indicated by the results reported, tends to confirm the supposition of clinicians that maladjustment is reflected in the expression

of needs which may be indicators of psychic tension. A tension index is a simply scored, relatively objective means of screening poorly adjusted from well-adjusted children. A word of caution, however, is in order for the clinician. Our findings suggest that the clinician may make serious errors in interpretation without specific normative data, both for the specific pictures and for the variables used in scoring. For example, our over-all project has shown that some variables are significant only for certain age groups, whereas other variables may be significant throughout the age range of 8 to 14, on which our test was standardized. The present paper has reported the rationale and method of developing our tension index with results for a common age group. Further study of our index with other age groups is under way. It is apparent that more accurate interpretation of projective tests of the TAT-type depends upon the accumulation of a considerable amount of such normative data.

Another important conclusion which may be drawn from the present paper is that our results show the analysis of TAT-type material can be greatly simplified and can be less time-consuming than is indicated in the literature to date.

Our results also suggest that psychic tension, as manifested simply by the expression of certain psychological needs in story material, is an adequate method for selecting poorly adjusted children without further regard for the intensity of the needs.

Finally, our results suggest the strong possibility that the Michigan Picture Test and the tension index may effectively screen maladjustment in school children.

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REVIEWS

EDITED BY STANLEY G. ESTES

SOCIAL PSYCHOLOGY. By Theodore M. Newcomb. New York: Dryden Press, 1951. Pp. xi+690. $4.75.

The vast wonderland that we call science becomes more minutely differentiated and more intricately organized day by day. To find our way about this amazing territory, we make use of maps; symbolic representations of the main features as abstracted by particular techniques, concepts, or observers. Now the cartographers have made it clear that a geographic unit has a different appearance according to the point chosen as a center when projecting spherical reality onto a symbolic plane surface. In the same way, textbooks give different representations of reality in accordance with the point upon which the system is focused.

Social psychology has certain features which make this analogy seem appropriate. Most of the schematic constructs designed to represent the operational realities of this area have not been developed on a system of coordinates centered on social psychology. Customarily, we have had an interpretation of social psychology either as a peripheral zone of sociology (Young; Krout; Lindesmith and Strauss) or as an outgrowth of individual psychology (Krech and Crutchfield; Allport; McDougall). But neither of these treatments does justice to certain aspects of the field. Just as biochemists had to develop some independence from biology and chemistry, so social psychologists need to center their coordinates on their own area, not on another—regardless of historical priorities.

Newcomb's *Social Psychology* represents an impressive attempt to do this job. The author has self-consciously set out to design a map of the territory which is focused on the *interaction* of individual with group. Both psychology and sociology are viewed as basic—and also, to some extent, as peripheral. While it would be an exaggeration to claim that Newcomb has been completely consistent in executing this design, the reader soon becomes aware of a patient re-ordering of data to fit the new coordinates.

Since interaction of individual with individual, and of individual with group, is the focus of the book, communication must become a key concept. Communication is impossible without group norms; the schizophrenic who rejects the group norms fails to communicate. Even objects derive their meanings from group norms; one's behavior toward a common object will communicate different meanings to varying observers, depending on their own norms (p. 270). "Reality" is determined by group norms (p. 295). At times I wondered if objects had any physical attributes whatever!

In view of this emphasis upon communication and the diverse meanings of objects and actions, I was disappointed to see so little use made of modern concepts of semantics. The importance of symbols was tacitly recognized but rarely pointed out. In a textbook, this seems a dubious practice.

Newcomb presents a sound basis in individual psychology for the phenomena with which he deals. His concept of motive pattern, including both need and an hypothesis as to the way it can be satisfied, seems versatile and useful. If one is startled to find a section headed "Perception as selective behavior" in a chapter on motives, he is soothed by the deft way in which it is integrated into the sequence of ideas. The concepts of perception and motivation, as combined in the motive pattern, are used in unorthodox but (to me) highly successful ways, e.g., in handling the Oedipus complex (p. 466).

Motive patterns become role patterns when the way of obtaining satisfaction is defined by the expectancies of other people. For this social definition of "proper" behavior, Newcomb coins the phrase "role prescription." As might be expected from one of his training, he is much more clearly conscious of individual variations in role behavior, and of latitude in the role prescriptions, than the general run of writers on social psychology (cf. especially p. 281). His treatment of this question is excellent. He is also realistic about the manner in which role prescriptions guide the child's learning process (p. 411).

Newcomb does not, however, avoid some of the contradictions which are likely to develop when one shifts from a focus on social role to one on the unique individual personality. On the same page (283) he alternates between emphasizing uniformity and recognizing individual variations in role behavior. This inconsistency is the fault of people, not of the psychologist; but Newcomb seems to avoid a frank facing of the problem.

One comes to value objects which are resources for the satisfaction of motive patterns. It is thus not surprising, he notes, that the self becomes a highly valued object. His treatment (p. 318) is, I think, somewhat more clear-cut than the recent discussions by Sherif and Cantril or Snygg and Combs.

Another valuable contribution in the theoretical realm is the distinction between goal-oriented and threat-oriented behavior. When the person reacts to frustration as simply a barrier which must be passed to reach the goal, we observe relatively constructive behavior. However, when the frustration is perceived as a threat to the self, the goal diminishes in importance and defensive behavior is manifested. This seems more accurate and fruitful than Maier's conception of frustration as leading to "behavior without a goal."

On the whole, Newcomb seems to have done a fine job of building a systematic interpretation of human behavior which is centered around the point of interaction between person and group. He has integrated some of the basic ideas of sociology and of psychology into a meaningful, coherent pattern.

Praise is also due for the thoughtful selection of factual material. While the book is primarily an exposition of principles and generalizations, experimental and field data are neatly placed at strategic points to put flesh on the theoretical skeleton. Furthermore, the data chosen have qualities of novelty, significance, and clarity. The interpretations rarely have to be forced; the application of the theoretical generalization is plain.

The prose style is clear and smooth. One might wish for a little more color; on the other hand, this volume is so readable compared to many others in the field that we should accept it gratefully. I suspect that college students will complain as to its difficulty, but on the content level, not the level of expression.

On the negative side I find two points which disturb me. The more important of these is the question of orderly arrangement. It seemed that the treatment of particular problems had been chopped up into discrete passages and pasted together in a none-too-systematic sort of way. For example, consider the treatment of "frame of reference," really a fundamental conception in this book which emphasizes perception and meaning so vigorously. This term is first introduced on p. 94—and not clearly defined, although the meaning emerges from the illustrations. It fails to appear in connection with "inclusive attitudes" (p. 129), but pops up again on p. 210. This passage, I think, belongs much earlier in the book. Then on p. 220 we learn something about group-shared frames of reference, but not until p. 264 do we hear about Sherif's ingenious study of the autokinetic phenomenon in a group setting—although Newcomb plainly considers this experiment as highly important for an understanding of the "shared frame of reference." And at last, on p. 288, he specifically concedes that reward and punishment are important in the determination of these ways of perceiving. I was also puzzled to note that he made no use of the "shared f.o.r." in his discussion of common role prescriptions (p. 424). Now none of these can be considered a major crime, but it seems that a much better job of tightening the organization and sequence of ideas could have been done.

Newcomb's training has been somewhat more in psychology than in sociology, and this may account for the fact that I felt his operations near the borderline of traditional sociology were often less than satisfactory. For example, he has little to say about institutions, and when he attempts a definition (p. 273), it does not seem too helpful. He mentions sociometry, but makes virtually no use of the method or concepts; in fact, the face-to-face group is rather neglected.

Since the book is rather long (690 pages), the author may have felt that he had to exclude some materials such as those mentioned. This leads to my second criticism of the book, which is that it is somewhat overwritten. My prescription for this would be a more thorough job of pruning on the present version. One gets a definite impression of repetitiousness in spots, none of it flagrant, but adding up to a general feeling that by proper organization and editing, between 10 and 20 per cent could be sweated out. This would leave ample space for inclusion of some of the missing topics.

It is easy to find minor quibbles. On p. 401 Newcomb introduces the phrase "out of focus" to identify the case in which the picture of self and the picture of others do not harmonize. The phrase seems inept if not actually misleading; each picture may be in focus, but the two are incompatible. Similarly, it is easy to take exception to his implication that "observation by a sociologist" is a criterion of "objective" reality (p. 277).

If we attempt to catalogue this volume as an example of a particular psychological "school," we encounter some difficulty. I think perhaps we need to resurrect the discarded term "functionalism" to identify Newcomb, along with Gardner Murphy, Sherif, and similar writers. There is more than a little of phenomenology and its predecessor, Gestalt. There is a fair sprinkling of a sophisticated behaviorism. Lewin's influence is apparent, although his conceptual contributions are avoided. Freud gets considerably less than what I would consider his fair share of attention, although a few of his ideas are skillfully reformulated. But throughout the book one constantly is reminded of C. H. Cooley, G. H. Mead, John Dewey, and William James. Nevertheless, this is no hodgepodge of eclecticism. The author has chosen wisely and well from his predecessors, and he has translated their ideas into his own concepts, viewed their data within his own frame of reference. This is possible because, as I have already noted, he has adopted a new set of coordinates for his map. By perceiving

all these phenomena as interactions of individuals, he is able to structure them in such a way as to achieve new and valuable insights into human social psychology.

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PERSONALITY: A SYSTEMATIC THEORETICAL AND FACTUAL STUDY. By R. B. Cattell. New York: McGraw-Hill, 1950. Pp. xii+689. $5.50.

Four major books on Personality have been published in America in the past 13 years. Their differences are considerable, yet probably no greater than those of four textbooks on general psychology picked at random. Cattell's is indeed not unlike Stagner's in the main fields of work with which it deals; and although poles apart from Allport's, yet, like his, it aims to provide a systematic and unified theory, rather than an eclectic survey. As might be expected from his previous publications, factor analysis plays a larger part in this than in other theories, since he hopes "to treat personality by the same scientific standards as are maintained in experimental psychology." He admits, however, the necessity of drawing largely on clinical, genetic, social, and other branches of psychology which are as yet chiefly at the descriptive stage.

Personality is defined as "that which permits a prediction of what a person will do in a given situation." At the moment, our knowledge is only rudimentary, our map of it as sketchy as a fifteenth-century map of the world. Nevertheless, one must agree that Cattell has done much to improve it by bringing together a very wide range of material from psychoanalysis and psychopathology, and from objective psychometric and sociological research. His standpoint is frankly "biophysical"; the view that personality depends largely on the observer, also the culture-pattern type of theory, are given short shrift, though several chapters are devoted to the influence of the family and other social groups on individual development. Little recognition, too, is given to the situational factors in personality or to the field theory approach.

Cattell is a strong adherent of McDougall's instinct and sentiment theory, but, more than most nonmedical psychologists, he draws on psychoanalytic concepts to supplement this. (His hereditarian trend is further shown by his claim that intelligence is more than 80 per cent genetically determined. One could pass this if he also mentioned that many psychologists believe the figure to be less than 50 per cent. But here and elsewhere his references to the literature, though very extensive, are somewhat selective.) One of the most valuable parts of the book describes the numerous ways in which constitutional factors become modified, so that a complex "lattice" or network of attitudes, sentiments, and complexes develops, many of whose strands may be unconscious. The final chapter summarizes these principles of personality formation in 17 laws, which are reminiscent of Hull's postulates. The end results of this process, including the Freudian Ego and Superego, the self-regarding sentiment, and the main syndromes revealed in the neuroses and psychoses, are believed to correspond to the "source traits" obtained by factor analysis.

The early chapters contain a summary of the material from the author's *Description and Measurement of Personality*, including the derivation of his 12 source traits from factorization of personality ratings. While the reviewer would agree that several of these represent important dimensions, which have often been found in other independent researches, he cannot see any good reason for accepting them all as valid descriptions of the fundamental traits of personality. Even though they have been rotated to Simple Structure, they still seem to him to consist of the underlying components of the subjects' reputations in the eyes of the raters; and the arguments for the biophysical status of any but B (intelligence) and C (emotional stability) do not convince him. Nevertheless, Cattell's scheme deserves very serious consideration, and its ramifications into psychosomatics, delinquency, changes due to age or to drugs, etc. are traced with great ingenuity. He also allows for the uniqueness of each individual, not only through the multidimensionality of source traits, but also by his *P*-technique which provides for factorization of unique traits within a single personality.

The book is well written, and illustrated with numerous helpful diagrams. Even the more technical sections should be clear to the graduate students for whom it is intended.

P. E. VERNON

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PUBLIC OPINION 1935–1946. By Hadley Cantril and Mildred Strunk, Editors. Princeton: Princeton Univ. Press, 1951. Pp. lix+1191. $25.00.

For twenty-five dollars (list price) the purchaser may buy a carefully edited, strongly bound miscellany of 1191 pages of polling results on topics ranging from *AAA* (see Agricultural Adjustment Act) to *Yugoslavia* (see Reconstruction; Russia; Politics and Government). The data have been collected from 23 polling organizations operating in 16 different countries and covers the period 1935–1946. The plan of presenting the data (which seems to have been the responsibility of Mildred Strunk) is based upon the Library of Congress subject headings with a sufficient number of cross references

provided in the Table of Contents to make the book as useful as it can be. The general rule which guided the editors for inclusion of data seems to have been: Include *almost* every question which has been asked by these 23 organizations on a *national* sample—and, as the publisher's blurb points out, this has meant including "literally every subject under the sun." I am adequately impressed with the thought and labor which have gone into making the resulting terrifyingly large mass of data of this collection readily available. What does not impress me adequately, however (at least not twenty-five dollars worth), is the value of the data thus carefully brought together.

The reason for publishing this "monumental compilation" (the words are the words of the publisher) derives from the numerous requests received by the Office of Public Opinion Research when it became known that this Office was accumulating archives of polling data for research purposes. It therefore occurred to Cantril and his colleagues that "a service might be performed both for present and future social scientists if we could somehow manage to put between two covers at somewhat regular intervals the available results of carefully indexed surveys." The present volume is the result. The hope of the editors is that historians, sociologists, political scientists, economists, editors, government officials, policy makers, and a host of others concerned with public reactions will find it useful in their work. The possible value of these data is indicated in Cantril's remark, "We thought of the interest comparable information would have for us today if it had been gathered during the period of the American or French Revolution, the Civil War, pre-Hitler Germany, or the early days of the New Deal."

Now there can be no question that the information in this volume will be of *interest* to the social scientists of the future—but of how much *value* will it be? To answer this question we must assess the validity and reliability of the data in this volume as *measures of opinions and attitudes*. And here the editors fail us. There is one brief paragraph in Cantril's "Preface" which touches on this. Cantril admits that "our methods for measuring and understanding public opinion are still in their infancy," but then goes on to say that we are improving our methods, research is being done, and that "careful questionnaire design and skilled interviewing provide one of the most reliable instruments so far designed for such research." Somehow I feel that there is a very large "Undistributed Middle" in this argument. I would agree that much research is being done. I would agree that "careful questionnaire design and skilled interviewing provide one of the most reliable instruments so far designed for such research"—but what has all that to do with the data in this volume? Almost all of the data come from members of the Gallup chain, and I doubt whether any social scientist would assess the "Gallup method" as incorporating "careful questionnaire design and skilled interviewing." Knowing what we do know about the methods of the Gallup Poll, the Roper Poll, etc., the social scientist who is not sophisticated in the field of survey techniques should have been provided with a very strong caveat by the editors. Neither am I impressed with Cantril's argument that ". . . the increasing amount of money spent by hard-headed businessmen . . . in acquiring special information via surveys can only reflect the demonstrated reliability of such data." It is just conceivable that "hard-headed" in this case may mean something other than that which Cantril wishes us to see in the term. It is also unfortunate that the exact size of the sample used for each question (and the nature of the sampling procedure) is not given. The editors say that to provide such data "proved impracticable," but there is no indication of why this proved impracticable. Lacking this information, the value of the data in this volume becomes extremely difficult to assess.

Now no doubt there are many useful and perhaps even reliable (although we will never know how reliable) data herein—census data which can tell us how many people in Denmark in 1940 lived in three-room apartments, etc. I don't want to underestimate the value of such information for many social scientists, but as "an indispensable storehouse of information" on public opinion (and *Public Opinion* is the volume's title), I have my doubts.

DAVID KRECH
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HANDWRITING ANALYSIS: AS A PSYCHODIAGNOSTIC TOOL: A STUDY IN GENERAL AND CLINICAL GRAPHOLOGY. By Ulrich Sonnemann. New York: Grune & Stratton, 1950. Pp. x+276. $5.00.

This book is based upon a sound appreciation of the critical attitude towards graphology current in American psychology, and of the exceeding difficulty that the clinical psychologist who uses a configurational approach to the analysis of personality productions must face in convincing a more quantitatively-minded psychologist of the validity of such a method. The author does not attempt to rationalize away these problems, but attempts to face courageously and straightforwardly the limitations of his method, while in a logical and dispassionate way advancing the theory and principles upon which his systematic analyses are based.

No one who reads this book will dismiss hand-

writing analysis as a superficial and simple procedure. That it demands artistry and must be both complex and time-consuming is readily evident, but that it is also a communicable method is clearly demonstrated by the author's thorough and detailed description of the various aspects of handwriting and of the personality factors operating in their production. The definition of the factors in handwriting is so well supported by illustrations of actual scripts that application of the analysis to new samples could be accomplished with considerable confidence by the psychologist who is willing to master the text and to accept the underlying theories upon which it is based. In this respect the text is outstanding as a communication of a clinical technique. The author's style, however, is not a simple one; the sense of many passages is only forthcoming after rereading.

The book begins with an introduction which sets the author's system of graphology in its historical framework as an outgrowth of Klages' studies of expressive movement, and which relates handwriting analysis to present concepts of personality theory, primarily those of Angyal. The basic tenet of the theoretical statement is that the expressiveness of every movement of the organism is attributable to a structural principle within the organism, to the meaning of the movement to the individual. Two categories of goals appear to the author to be in operation in expressive movements: (1) the *purposive* goals, the conscious purposes of the organism in the production of the movement, an outgrowth of Angyal's dimension of progression "in which personality functions are determined according to the law of finality underlying their temporal order in the conscious pursuit of goals"; and (2) the *expressive* goals, the release of unconscious organismic impulses, the product of Angyal's dimension of depth "in which personality functions are determined according to the unconscious organismic needs they correspond to." A third category, identified as *aspiration*, appears to result from a conflict between the above two goals, i.e., when conscious attention is diverted towards the self, a representation of Angyal's transverse dimension "in which personality functions are determined according to their order of mutual coordination." Sonnemann regards handwriting as a system of expressive movements of particular psychological value because of the balance potentially found between purposive goals (the content of the communication) and expressive goals (the individual manner of communication) and because of the record of their operation left in the handwriting trail.

Perhaps it is understandable that the theoretical point of view of a psychologist concerned with motor expression should be dominated by concern with intra-organic phenomena. But one misses in Sonnemann's approach a sensitivity to the situational factors that may affect handwriting. While he does make a passing reference to the fact that samples produced spontaneously are of greater value than those made by copying or for the immediate purpose of graphological analysis, he does not show either in his theory or in his interpretations the importance of taking into account the sensitivity of movement to the circumstances under which it is made. This is the more surprising since his analyses are consistently based on the recognition of the specimen of writing as a Gestalt where single movements gain meaning from their relationship with the unity of the writing as a whole.

The main contribution of this volume is found in Part II, where (1) the basic concepts of the graphological method, (2) the interpretative principles involved in analysis of the script as a whole and in its various dimensions, and (3) the methods of procedure in the development of personality descriptions based on handwriting are defined. In this part Sonnemann first describes three global characteristics of handwriting: the level of form quality, the rhythmical qualities, and the over-all arrangement. This provides the context within which interpretations of the specific dimensions of the graphic movements appear. These dimensions, described as the temperamental (extension, velocity, and impact), the directional (verticality and horizontality), the ductual (forms of binding), and the background involvement (amplitude, enrichment, and dispersion) aspects of movement, are linked in a series of tables with positive and negative trait-meanings with which they are seen as capable of association. The identification of the writing characteristics does not permit development of a personality picture by direct reading from the trait-lists, since the interpretation is dependent upon the total configuration within which the dimension is discovered, and since the indicators in the writing are regarded as having a range of meaning from extremely positive to extremely negative, depending on their context.

Part III of the book is composed of illustrations of handwriting analysis. A rather full graphological study of a 30-year-old woman illustrates the interpretative method. A second comparative study of four handwriting samples shows the use of graphology in vocational selection. After a definitive treatment of handwriting characteristics in psychiatric disorders in general, and affective, schizophrenic, psychoneurotic, intracranial, and miscellaneous disorders in particular, amply supported by reproductions of writing samples, ten brief clinical cases are considered. While the case materials are well developed from the point of view of demonstrating the interpretative method used by Dr. Sonnemann, the lack of supporting independ-

ent clinical material leaves the reader with no other check on the validity of the interpretations than the authority of the writer.

Those who are familiar with the European sources on handwriting, especially the writing of Klages, will find much that is familiar in this treatment of graphology. This presentation is no mere translation, however, and it expands and advances both the theoretical and practical aspects of graphology in such a way that we can easily overlook the few errors in names in the text and bibliography. In addition, it makes available for English readers a technique that, as Bela Mittelmann has said in his Foreword "... can take its place as a full equal in personality diagnosis with other projective methods, e.g., the Rorschach test ..."

JOHN E. BELL

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CHILDHOOD AND SOCIETY. By Erik H. Erikson. New York: W. W. Norton & Co., 1950. Pp. 397. $4.00.

Though not long, this is a book of wide scope and stirring vistas. Its starting point is the study of children undergoing psychoanalytic treatment, but pathology is used only as an avenue to the understanding of normal development. The author successfully keeps in view not only the failures but also the constructive achievements of the youthful ego. Having this outlook, he passes beyond the clinician's almost exclusive preoccupation with the family circle and examines the relation of the ego to the larger society. This puts him in a position to appraise the helpfulness of the psychoanalytic theory of development in understanding the problems of the modern world. The bridge from childhood to society would quickly buckle if it were merely an unsupported extension of psychiatric concepts. Here its foundations are solid, for it is the handiwork of a man whose rare gifts for observation and reflection have not been confined to his specialty but have been turned also to anthropological field work and to a sensitive study of society and history.

The main theme can be summarized as follows. Prolonged childhood dependence is essential for the big job of learning which every infant must eventually accomplish, but it carries the liability that bothersome residues of emotional immaturity will persist into adult life. In one way or another all societies tend to exploit the child's helplessness and anxiety in order to shape him to a cultural pattern, thus intensifying certain residues of immaturity. The effects of such residues are clear enough in neurotic conflict, which Erikson calls "the most comprehensively studied aspect of human behavior." Of greater concern in this book is their effect on man's attempts at mature accomplishment and the solution of social problems. Their influence can be properly grasped only after studying the development of the ego, for maturity and judiciousness must be understood as well as the forces which betray them. With respect to the betraying forces, however, "the problem to be elucidated is that of the extent to which man is apt to project on political and economic necessity those fears, apprehensions, and urges which are derived from the arsenal of infantile anxiety." It is such projections, subtly undermining the efforts of the mature ego, which lay the adult open to exploitation all over again by the prevailing power groups of his time.

The importance of this theme is obvious, and it is here treated with rare distinction and vividness. Mentioning his early training as an artist, the author apologizes a little for basing what he has to say "on representative description rather than on theoretical argument," but to me this anchorage in real examples and real facts is one of the strongest virtues of the book. The case material, both of individuals and of societies, is painted by the deft brush of an artist in the use of words. Most of the illustrations have already been published in journals and monographs: for example, the studies of play and of child analysis, the field trips to the Sioux and Yurok, and the analysis of Hitler's imagery. What is added here is an over-all theme, a search for those general meanings toward which the separate observations point. Erikson calls the book in one place "a conceptual itinerary," and it indeed serves as such not only for its author but for all readers who are interested in both childhood and society. The unifying theme is found in the development of the ego, especially of ego identity, and the relation of the ego to society.

One of the most substantial sections of the book is Erikson's persuasive restatement of the libido theory. For expository purposes he uses a series of diagrams to represent the zones (oral, anal, genital) and the organ modes (incorporative, retentive, eliminative, intrusive) which are prominent in infantile development. Zones and modes are treated as at least partially independent variables, and the concept of mode can be applied to intellectual activities and social relationships as well as to bodily functions, thus giving the libido theory that expanded power of explanation which seems so convincing to practicing psychoanalysts, however arbitrary it may appear simply as theory construction. The third part of the book, on the growth of the ego, makes a material addition to the theory of psychosexual development by stating explicitly the kind of emotional problem that is associated with each stage and showing its effect on growing ego identity. Thus the earliest experiences of feeding and care provide either basic trust or mistrust;

bowel training may strengthen the sense of autonomy and free choice, or it may develop feelings of shame and doubt; puberty brings the chance for strengthened identity but the danger of role diffusion; and so on, in each of the "eight stages of man." Here Erikson broadens the Freudian conception in such a way as to make it more adequate for explaining normal adult life. The psychology of the ego, long of interest to the psychoanalytic school, is definitely carried forward.

It seems to me that Erikson has something particularly important to say in his discussion of ego identity (pp. 207–213) and his applications of the idea in later parts of the book. A sense of identity and of self-esteem is built from accomplishments, beginning with such elementary ones as standing and walking. Adult praise and encouragement cannot fool the child if he does not feel that his accomplishments are real and have some importance in the scheme of things. This poses a special problem in the push-button age when so few useful actions beyond pushing buttons are within a child's competence. More broadly, a strong sense of identity depends upon being able to perceive what one does as contributing to meaningful social roles and social purposes, a sense of meaningfulness which the parents, too, must have if they are to contribute to its production in their children. The effect of conditions adverse to ego identity is examined in the American Negro child and the Sioux Indian child. But even for children more favorably situated the confusions of modern society constitute a hazard in developing a strong sense of identity.

Erikson's views on the ego lead him to reinterpret the meaning of children's play. "The child's play," he proposes, "is the infantile form of the human ability to deal with experience by creating model situations and to master reality by experiment and by planning," the same kind of thing that later precedes man's most constructive work and highest accomplishments. Clinicians have seized upon play as a means of diagnosing neurotic conflict, but the more broadly oriented student of human nature needs to realize that most childhood play is an activity of the developing ego and an important step on the way to maturity.

This book is not a systematic treatise. It does not attempt to create a new theory or to propose an inclusive conceptual scheme. It is not a book of knowledge, but it is a book of great wisdom. I would recommend strolling fairly slowly through its pages so as to appreciate the many treasures that lie along the path. One should not overlook, for example, the author's exquisite skill in integrating the diverse influences that operate in any given case: somatic, ego, and societal processes contributing to the seizures of three-year-old Sam; personal dynamics and historical-social forces converging to produce the rise of Hitler; likewise with all the illustrative material that makes the book so readable. It is a stimulating book, and with its broad horizons and seasoned reflection not without a cerpower to promote ego development in the reader.

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EXPERIMENTS ON MASS COMMUNICATION. By Carl I. Hovland, Arthur A. Lumsdaine, and Fred D. Sheffield. Princeton: Princeton Univ. Press, 1949. Pp. viii+345. $5.00.

This volume is one of a series describing the work of the Research Branch of the Army's Information and Education Division in World War II. The book summarizes a number of experimental studies concerned with the effectiveness of mass communication devices, particularly films, for spreading information and changing opinions.

The contributions which this report makes to the area of mass communications are twofold: (1) it gives well-documented empirical answers to questions of practical application; (2) it advances in important ways the theory and methods of this field. The authors' sustained interest in problems of general theory and basic methodology is, indeed, an outstanding feature of this book. In a research situation which necessarily put heavy emphasis on findings of immediate practical utility, Hovland and his colleagues succeeded in formulating their problems so as to give them maximum generality and continuity with other areas of psychology.

To what extent and under what conditions can mass communication devices disseminate information, change opinions, and affect motivation and morale? To find answers to these questions, two general types of research design were employed: (1) evaluative studies and (2) controlled experimentation. Evaluative studies gauge the effects of a finished product, such as a film or a radio program. In controlled experiments the nature of the message and its conditions of presentation are systematically varied in order to discover the determinants of effective communication. The conclusions from evaluative studies are necessarily limited to the specific products which were tested in a given investigation. Controlled experiments, on the other hand, may produce conclusions of general validity. To the extent that such controlled experiments are grounded in psychological theory the generality of the conclusions is enhanced. In the long run, therefore, the controlled experiments will produce a wider range of practical applications. The work reported in this volume clearly illustrates the superiority of controlled experimentation over evaluative surveys, and the authors effectively underline this conclusion from their experience.

The evaluative studies did, however, provide good opportunities for developing and refining survey methods. Considerable effort was devoted to the development of testing procedures and techniques of measurement for gauging the changes which training films and related devices produced in Army personnel. Primary emphasis was on opinion change and acquisition of information, and the criteria of evaluation were concerned with the amount of learning produced by exposure to the messages. Criterion measures (answers to information and opinion items) were related to such characteristics of the audience as intelligence and initial opinion, to the contents of the message, and to the conditions of administration. The results of the evaluation surveys indicate that training films are highly effective in spreading knowledge of factual material and in changing opinions with respect to issues specifically discussed in the films. The effects of films are negligible, on the other hand, in affecting general opinions and beliefs, nor do they have any effect on morale as measured by the subjects' motivation to serve as soldiers. These conclusions are necessarily limited to the specific films investigated in these surveys. The great care used in developing information tests and scales of opinion for measuring the effects of the films does, however, give these results the maximum validity possible under the circumstances. As the authors point out, results such as these raise serious doubts about the possibility of changing deep-seated convictions and motivations by means of factual information.

The controlled experiments reported in this volume may be regarded as investigations in the applied psychology of learning. Hypotheses were tested concerning the effects of specific variables on acquisition of information and opinion change. The nature of the messages and the conditions of presentation were systematically varied, and resulting effects on criterion measures were observed. Some of the factors which were so investigated under conditions of practical application were variables previously studied in the experimental laboratory. In demonstrating the beneficial effect of active audience participation and of incentive on the acquisition of a verbal skill, for example, these experiments verified the results of laboratory studies. At the same time, these studies succeeded in going well beyond the earlier laboratory findings. In the study just mentioned, the relations between active participation, motivation, and intelligence as determinants of learning were explored for a wider range of ability and under more realistic conditions than are ordinarily available in the laboratory. These experiments effectively illustrate the fact that the dividing line between academic and applied research need not be a hard and fast one and that the possibilities of mutual benefit are considerable.

Another experiment evaluated the effects of presenting one side versus presenting both sides of a controversial issue on changing opinions. Presentation of both sides is clearly more effective among those who are initially opposed to the point of view of the communication than among those who agree with it to begin with; it is also more effective among the better educated than among the less well educated. This study shows how survey techniques (measurement of initial opinion and education level) can be usefully combined with controlled variation of procedures (presentation of one side vs. both sides) to arrive at generalizations about the most effective methods of teaching by mass communication.

The book is written in a clear and lucid style. The empirical material is well summarized, and the reader is greatly aided by the many graphs and charts. An appendix contains detailed discussion of problems of experimental design and statistical techniques which will undoubtedly be of great value to investigators in the area of mass communication. This report may well serve as a model for experimental psychologists faced with problems of practical application.

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LOVE IS NOT ENOUGH: THE TREATMENT OF EMOTIONALLY DISTURBED CHILDREN. By Bruno Bettelheim. Glencoe, Illinois: The Free Press, 1950. Pp. xi + 386. $4.50.

This book gives a vivid picture of the behavior and treatment of a group of emotionally disturbed children at the University of Chicago Sonia Shankman Orthogenic School. Procedures are based primarily upon a psychoanalytic approach. This theoretical orientation and the underlying philosophy influences decidedly the methods used in dealing with these children. As one reads the book, filled as it is with interpretations and evaluations of the procedures used, one becomes more aware of the influence any basic orientation and philosophy has upon practices and interpretations. We definitely see what we are looking for. Probably one of the most significant contributions of this book is the picture that it gives to the reader of the confused, unhappy, miserable feelings of emotionally disturbed children, children whose early lives have been so warped that they are unable to make peace with themselves and with one another. Certainly another valuable contribution will be the challenge in this book to raise questions for critical examination and study in regard to the practices and interpretations that are related in detail throughout this book. A systematic follow-up study to determine the results

of these therapeutic and educational procedures would be of great interest to many readers, especially those readers whose basic philosophies and theoretical orientations differ sharply from the psychoanalytic approach and whose evaluations of the procedures discussed in this book are at variance with the author's.

Acceptance or rejection of any interpretation or method needs to be based upon a psychological rationale. If we proceed in a certain way in working with an emotionally disturbed child, we should have sound psychological reasons for doing so, and even then the results should be critically evaluated. If we disagree with certain procedures, it is not enough to say "Not that!" We need to give an adequate reason for the disagreement and to suggest alternative procedures. In either case, agreement or disagreement, the individual's understanding of why he behaved in the way he did seems to be of real value in order to insure a scientific, objective, constructive method of increasing our understanding of the problems we face and of applying more effective methods of handling them as this understanding develops. I am referring to the counselors' and teachers' understanding of why they behaved as they did with the children. Mr. Bettelheim does not give us a very clear-cut or convincing picture in respect to the psychological rationale behind the procedures used. It would have been of great interest to know what psychological training the counselors and the teachers had had since the therapeutic burden was obviously in their hands. It is the counselor who gives support, reassurance, approval, interpretations, who probes, who does whatever is deemed best for each child at the moment. Such a tremendous responsibility certainly should imply that there had been extensive psychological training to prepare them for such work; and yet the reader gains the impression that the counselors are young students with most of their training received as observers and participants at the School, where they received their orientation. Since such short-term training for therapeutic work is generally frowned upon by both psychiatrists and clinical psychologists, Mr. Bettelheim's method of training these counselors should be of extreme interest to both groups. No matter what label is attached to the work of these counselors at the School as related in the book, they *are* doing psychotherapy. It is the *treatment* of emotionally disturbed children. Since it is described as successful then what part of it would be applicable in any educational experience for children? And how does the counselor-teacher learn in such detail what the child "wonders" and "feels" in those instances where he does not verbalize it? And how do the counselors develop the "empathy and intuition" that tells them when to listen to a dream, when to probe, when to interpret, when to encourage, when to sidetrack a feeling, and when to approve?

I am sure it was not Mr. Bettelheim's purpose to present a picture of the training program, but, rather, to give a frank and honest account of what occurs in the school every day. However, those things that do occur and the methods used are of sufficient significance to the lives of many people that this reviewer, at least, would have appreciated material in the book to justify the unquestioned procedures cited throughout the book. The counselors were never wrong.

By way of example, without adequate explanation as to how they knew what the children "wondered" and "felt," the book is full of such statements as the following: On page 41, "By then he began to wonder if we might not, contrary to his expectations, be more interested in allowing him the freedom to behave in line with his emotional need to waste food, than we were in saving food or in enforcing good manners at the table." That's pretty good insight for an emotionally disturbed child to have at his "first meal." On page 318 this is cited: "It was some time later when he was having a bath one evening, that he re-enacted what looked like a seduction by his counselor and followed that by pretending to castrate himself. But while his movements showed his penis being sawed off, what he said was, 'Oh, now you cut off my arm!' So his counselor said to him matter-of-factly, 'You know, Richard, I'm not your mother.' There was momentary shock, but immediately afterwards Richard's reactions showed some first signs of response to her as a woman distinct from his mother, and a person who was trustworthy." On page 84, "Only intuition and empathy will help the counselor to encourage those children to review their dreams whose egos are strong enough to face and work through the dream material—most often their anxieties—and to see to it that others do not. The counselor must be guided by his often unconscious impressions of how strong the child is at the moment, or how shaken he seems to be as he thinks about his dreams."

This would imply that if the counselor decides the child should not discuss his dreams it will not bother the child. Some psychologists believe that such forced repression because of the counselor's "unconscious impressions, empathy, or intuition" would be harmful to the child and would not necessarily mean that it had disappeared at the bidding of the counselor.

The concept that one can deny, divert, or reassure a feeling out of a child occurs throughout the book.

One other practice impressed this reviewer: the encouragement and approval given to children to behave in certain ways about which the children showed and expressed anxiety and fear. For exam-

ple, on page 347, "After a few weeks at the school, all children understand that bedwetting is a symptom like any other, neither more serious nor more obnoxious. As a matter of fact, we sometimes go far beyond this protection of the bedwetter and his 'right' to wet his bed, and find it necessary to encourage a child to do so."

And on page 357, "What apparently worried Emily now was what her counselor's attitude toward masturbation might be, whereas before what her counselor approved of or didn't approve of had been less important to her. The counselor assured her repeatedly and in unvarying terms that masturbation was not harmful, until one night her talk had an unusual sequel." The "sequel" is of significance to any therapist who does not follow this particular counselor's orientation.

Other methods used in the treatment of these children are thought-provoking and challenging. Although readers will differ greatly in their reactions to this material, it forces the reader to clarify his own point of view.

One might ask if we sometimes are not feeding symptoms when we think we are satisfying needs. There is a definite position taken in this book that the most effective way of handling the problems is at the source of the activity—toilet problems in the bathroom, sex problems in the nude, eating problems at the table, running-away problems on the run. Any symbolic methods of dealing with the problems are belittled and a therapeutic period that is set aside for such expression is considered inadequate. There are inconsistencies throughout the book in regard to these statements.

The parents are kept as completely away from the child and as apart from the treatment as possible—and yet they are considered to be the main source of the children's difficulty. On page 344 we read, "Although the child does not consciously know who the person is he is so afraid of, or for whose safety he fears, the very fact that the School is geographically removed from that person does a great deal to quiet his night fears." On page 353, Jack symbolically plays out one of his problems in bed with his toy animals.

It is said many times, "Our statements alone they do not trust," and the author decries the use of verbalism, but cites numerous examples of reassurance, approval, encouragement, interpretation on a strictly verbal level. Then on page 290 we read, "Tentatively it may be suggested that non-verbal contacts reach into the deepest and earliest layers of the personality, layers that were developed before speech."

All in all, the reviewer would recommend this book to all people who are interested in the treatment of emotionally disturbed children. It certainly will not be read with apathy.

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NEW BOOKS RECEIVED

ALLPORT, GORDON W. *The nature of personality: Selected papers.* Cambridge, Mass.: Addison-Wesley Press, 1950. Pp. vii+220. Price $2.50.

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